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PHILOSOPHICAL INQUIRY INTO THE
LANGUAGE AND METHODOLOGY OF HISTORY

by

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The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled Philosophical Inquiry into the Language and Methodology of History submitted by Stanley John Paluch in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

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ABSTRACT

In this thesis I examine contemporary philosophical arguments which are designed to clarify the nature of the relationship between history and the sciences. Generally speaking, philosophers who have sought to clarify the nature of the relationship between history and the sciences have either drawn the conclusion that history, from a methodological point of view, stands apart from the sciences (having its own means of giving us knowledge) or have drawn the opposite conclusion that history shares a common methodology with the sciences.

In recent decades these opposed conclusions have proven as resistant to demonstration or disproof as they were when the discussion about the nature of history really got started in the nineteenth century. Some may draw the conclusion that either the "positivists" (as those are often called who consider the methodology of history to be scientific) or the "idealists" (those who believe

that history has a methodology quite distinct from that of the sciences) must be mistaken and that with progressive clarification, and continued discussion, one side or the other will be vindicated.

Another possibility--one which is developed and defended in this thesis--is the possibility that the debate continues because (a) the discipline of history has both scientific and non-scientific aspects and (b) a history could be produced--if only, as I argue, in a truncated form--which would be exclusively "scientific" (presupposing or consuming laws as the only legitimate basis for explanations) or exclusively "non-scientific" (offering only "rational explanations" of human behavior.)

History as we have it, the actual practice of historical research, is a mixture of art and science. To take sides in the debate between the "positivists" and the "idealists" is to commit oneself to the proposition that one part or the other of this "mixture" should be minimized or eliminated. However, if one part or the other of this "mixture" is eliminated the scope of history is narrowed. History fails to cover either the rational, or the non-rational, dimension of human behavior. In either case it

no longer serves to mirror human existence and those human actions which have been adjudged to be of historical significance.

Historical practice reveals a resistance to the doctrines of both "positivists" and "idealists." The kinds of explanatory patterns historians accept (both "covering law" and "rational") reveal a tolerance which rests on the insight that less tolerance would lead to "truncated" histories which would not adequately mirror human existence in both its rational and non-rational aspects.

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PART ONE

CHAPTER I

INTRODUCTION

Since the time of Hegel philosophers of all schools of thought have been concerned with the discipline of history. It is generally agreed that there is something unusual about the historian's work which makes it difficult to place history on what may be referred to as the epistemological map. For one thing, the ways in which historians give us knowledge of the world do not appear to fit in with the explanatory patterns we discern in the work of the scientist.

It is generally admitted that the historian deals with what no longer exists in terms of more or less specific descriptions of individual events.¹ In the words of

¹Not all philosophers of history would agree with this seeming truism. There are those who argue that the historian may be as concerned with the future as with the past. Maurice Mandelbaum has argued in this vein in his The Problem of Historical Knowledge (Liveright Publishing Co., New York, 1938), pp. 11-3.

Alan Bullock, the historian's "purpose is not to form general propositions about revolutions or civilizations as such, but to give an account of the French or the Russian Revolution, to trace the rise and fall of the Hellenic or Chinese Civilizations."²

Professor Bullock's statement of the historian's purpose recalls Wilhelm Windelband's famous distinction between nomothetic science and idiographic science. A nomothetic science has as its goal the formulation of general laws whereas an idiothetic science has as its goal the description of individual events:

So duerfen wir sagen: Die Erfahrungs-
wissenschaften suchen in der Erkenntnis des
Wirklichen entweder das Allgemeine in der
Form des Naturgesetzes oder das Einzelne in
der geschichtlich bestimmten Gestalt; sie
betrachten zu einem Teil die immer gleich-
bleibende Form, zum anderen Teil den
einmaligen, in sich bestimmten Inhalt des
wirklichen Geschehens. Die einen sind
Gesetzwissenschaften, die anderen
Ereigniswissenschaften; jene lehren was
immer ist, diese, was einmal war. Das
wissenschaftliche Denken ist--wenn man

²Alan Bullock, "The Historian's Purpose: History and Metahistory," History Today, Vol. 7. My citation is from the reprint of this article in Hans Meyerhoff, ed., The Philosophy of History in Our Time (Doubleday Anchor Books, Doubleday & Co., Inc., Garden City, New York, 1959), p. 296.

neue Kunstaussdrücke bilden darf--in dem einen Falle nomothetisch, in dem anderen idiographisch.³

History, because it teaches us what once was, is an idiographic science. Although Windelband, like Bullock, is justified in calling attention to the fact, sometimes ignored or underplayed, that the historian is not primarily concerned with formulating laws or generalizations, the weight he comes to place on his distinction as a methodological means of drawing a boundary line between history and theoretical science seems unjustified.

The mere presentation of material does not insure that the reader will understand this material or be in a position to weigh its significance. Windelband's classification of history as an idiographic science would be satisfactory only if the historian limited himself to "an exact description of what happened, constructing what may be called a plain narrative of past events."⁴ But as

³Wilhelm Windelband, "Geschichte und Naturwissenschaft," Praeludien (Verlag von J. C. B. Mohr, Tübingen, 3d ed., 1907), p. 364. This article is a reprint of Windelband's Strassburg rectoral address, first given in 1894.

⁴W. H. Walsh, Philosophy of History: An Introduction (Harper Torchbooks, Harper & Brothers, New York, rev. ed., 1960), p. 31.

W. H. Walsh, among others, has pointed out: "The historian is not content to tell us merely what happened; he wishes to make us see why it happened, too."⁵

Windelband touched on this point, obliquely, but failed to see its relevance for his classification of history:

Der notorisch aeusserst unvollkommene Grad, bis zu welchem bisher die Gesetze des Seelenslebens haben formuliert werden koennen, hat den Historikern niemals im Wege gestanden: sie haben durch natuerliche Menschenkenntnis, durch Takt und geniale Institution gerade genug gewusst, um ihre Helden und deren Handlungen zu verstehen.⁶

The historian has an understanding of human nature which permits him to interpret the past.

The historian, through tact, intuition, and his grasp of human nature, is able to understand the actions of his objects. But if the historian can understand these actions he must, in some way, be able to explain them. Windelband does not show how the historian's ability to explain the past is compatible with a purely descriptive approach.

⁵Ibid., p. 32.

⁶Windelband, op. cit., p. 376.

Some philosophers have argued that Windelband's distinction between nomothetic and idiographic science is really quite similar to the distinction between pure and applied science. The historian describes specific happenings but his explanations dealing with the causes of these happenings make use of general laws:

History is comparable not to the sciences proper but to practical activities like engineering. In each case general knowledge is involved and applied, but in each case the centre of interest is in the particular specimen under examination. And if it is asked what general knowledge is in question, the answer is that that depends on the kind of history. Historians use general knowledge of every kind, trivial and technical, according to their subject-matter. But there is no case where they reach conclusions without appealing to general propositions at all.⁷

It is tempting to view history as a discipline which is methodologically similar to an applied science such as engineering or geology. This view does no injustice, on the face of it, to the historian's concern with description and, on the positive side, it represents a possible account of how historians are able to "link" the data which they introduce into their narratives. Further-

⁷Walsh, op. cit., p. 46. The material quoted above does not represent Professor Walsh's own view of history. It forms part of a general summary of the views of others.

more, as Walsh brings out, this view has:

. . . the attraction of all positivism: its avoidance of mystery-mongering. History, to judge from the way some philosophers speak of it, is a thing about which we all ought to be particularly solemn, since it offers that individual knowledge which other sciences pursue in vain. But the positivist theory, especially in its later form, removes all ground for such solemnity. It sees nothing peculiar in the fact that the historian is concerned with particular events: so are we all in our daily lives. And it argues that historical understanding involves precisely the same reference to general truths which is made in any deductive argument. Thus historical thinking can be seen to possess no peculiarities of its own, but to be in principle one with scientific thinking. History is not a science, but equally it is not an extra-scientific source of knowledge.⁸

The "positivist theory" has the merit of assimilating historical knowledge to the pattern, or patterns, exemplified in scientific knowledge. If it were obviously correct there would be no need of a separate analysis of the "logic" of historical explanation.

The philosopher of science who provides us with the correct model, or models, of scientific explanation would, by the same token, provide us with the correct

⁸Walsh, op. cit., pp. 46-7.

model, or models, of historical explanation.⁹ However, before we can profitably consider whether or not this view is correct, we must reflect on the implications of treating history as methodologically similar to an applied science.

If the methodology of history is of the same type as the methodology of an applied science history must have certain abstract properties in common with the applied sciences. Its concepts, like those of any applied science, must be "borrowed." Its claims must be subject to intersubjective methods of verification. Its explanations must make use of general laws.

If there are specifically historical concepts history cannot be depicted as simply applying the general knowledge which other fields, or "common sense," supply. If history has its own concepts history must, in virtue of that fact, possess some degree of autonomy in the shape of a specifically historical theory which permits the introduction of these concepts.

In any science it must be possible, at least in

⁹William Dray, Philosophy of History (Prentice-Hall, Inc., Englewood Cliffs, N.J., 1964), p. 2.

theory, to confirm or infirm the statements made in that science. The language of a science must be capable of expressing sentences which--when employed in the making of statements--can be assigned truth-values or can be supposed to have truth-values which are, for the present, unknown.

If historical statements, or some indispensable proper subset of historical statements, are immune to intersubjective methods of verification (or falsification) history lacks a property we associate with any science, pure or applied.

The theoretical sciences make use of general laws in the sense of supplying them. The applied sciences apply these general laws to specific cases. This is, of course, an artificial picture. The theoretical scientist discovers laws which make explanations possible. He must--to assure himself that he has made a genuine discovery--show that his discovery fits the specific cases to which it is relevant. His primary interest is in the law rather than in the specific case. For the applied scientist the primary interest is in the specific case.

If history does not make use of general laws, and

yet offers acceptable explanations, it must--even if it should possess the first two properties I have mentioned--differ in its methodology from any science, pure or applied.

If the modern positivist view of history--the view of philosophers of science such as Ernest Nagel, Carl Hempel, and Karl Popper--is the correct view, it follows that history either possesses these three properties or that progress in history is to be measured in terms of the degree to which history, assuming that it has no theory of its own, develops objective techniques and succeeds in making connections between laws and its data.

Very few people would say that history now has all of these properties in the sense in which, say, geology has them. While the modern positivist would hold that the first property is assignable to history,¹⁰ he would be prepared to admit that the laws of interest to the historian are not easy to characterize. He would also admit that moral judgments and personal points of view (presumably transcending empirical intersubjective methods of verification) play a role in history which they do not play in the

¹⁰Some positivists would treat this as a theoretically open question. See footnote 11.

"hard" sciences. But this admission would be qualified by the argument that the vagueness of the laws employed in history and the presence of moral judgments in historical writings are to be called defects of historical inquiry and are not to be considered signs of a special methodology.

In studying the nature of the relationship between history and science one should, therefore, make a distinction between actual historical practice and possible historical practice. The positivist does not claim to give us a description of historical practice; he claims to show us what historical practice, at its best, would be. He can justify this approach by pointing out the fact that a handbook of scientific procedure does not primarily concern itself with the failures and crude beginnings of a science but rather with the essential goals of the sciences and the ideal means of attaining these goals. With respect to history his claim is that insofar as history does give us knowledge of the past it can be seen to conform to certain methodological principles which are identical with the methodological principles of an applied science. (Some modern positivists are prepared to admit that there may be specifically historical laws but this admission has

played no role in their analyses of history.)¹¹

In this monograph I shall be concerned with philosophical arguments which are designed to show that history does have (or might come to have) or does not have (or can never come to have) the three properties to which I have referred. Arguments dealing with the first and second properties will be discussed in the first half of the thesis and arguments dealing with the third property will be discussed in the second half.

It is my contention that a study of the relationship between historical methodology and scientific methodology will reveal that history is a mixture of art and science. By this I mean to say that I believe that history provides us with more than one kind of knowledge and more than one kind of explanation. I appropriate the word "art" because it has often been held that the artist gives us knowledge of the world without making use of scientific

¹¹Carl G. Hempel, "The Function of General Laws in History," Journal of Philosophy, Vol. 39. Nonetheless I think one is justified in saying that the positivist equates history, in terms of its methodology, with the applied sciences. Since no specifically historical laws are known, or vouched for by any positivist, in practice positivists treat history as if it were like an applied science.

techniques. I simply use the word "art" to refer to any non-scientific (not anti-scientific) means of gaining knowledge. I hope to show that a positivistic, or scientific, history would be more restricted in its scope and usefulness than any history we now have. At the same time, I shall try to show that a completely non-scientific history would also represent a loss in scope and usefulness.

If I am right in saying that history is a mixture of art and science and that only this mixture can give us what we expect of history, then it follows that exclusive emphasis upon one or the other part of the mixture, in a philosophical discourse on the nature of historical inquiry, produces a distortion. The distortion does harm to the historian--if any--who tries to live up to the terms of whatever philosophical discourse, from the scientific or the non-scientific side, which he has read. I hope to show, primarily in the eighth chapter of this monograph, that this distortion not only represents an impoverishment of our historical perspective but also an impoverishment of our perspective of human existence.

CHAPTER II

THE VOCABULARY OF HISTORY

Morton White, in his well-known article, "Historical Explanation," attempts to show that there are no unshared, specifically historical concepts and that, in virtue of this fact, history has at least one property in common with the applied sciences; it lacks a theory proper to itself. (I shall explain the notion of an "unshared concept" in the course of the discussion.)

Professor White introduces the notion of what constitutes an essential occurrence of a term (or concept represented by that term) in a scientific discipline:

. . . every theory can have its statements divided into two classes--those which are statements of the disciplines presupposed by the theory, and those which are specific to the theory . . . We said, it will be recalled, that an explanation of S is an explanation every one of whose constituent statements is a truth of S. We should revise that to read that it is an explanation every one of whose constituent

statements is specifically a truth of S. What, now, is such a truth? It is one that contains some specific terms of S in an essential way.¹

He has, as he tells us, taken over this notion from Quine. Quine gives the following description of the use of the expression "essential occurrence":

An expression will be said to occur vacuously in a given statement if its replacement therein by any and every other grammatically admissible expression leaves the truth or the falsehood of the statement unchanged. Thus for every statement containing some expressions vacuously there is a class of statements, describable as vacuous variants of the given statement, which are like it in point of truth or falsehood, like it also in point of a certain skeleton of symbolic make-up, but diverse in exhibiting all grammatically possible variations upon the vacuous constituents of the given statement. An expression will be said to occur essentially in a statement if it occurs in all the vacuous variants of the statement, i.e., if it forms part of the aforementioned skeleton.²

¹Morton White, "Historical Explanation," Mind, Vol. 52. Reprinted with revisions and additions in Patrick Gardiner, ed., Theories of History (The Free Press, Glencoe, Ill., 1959), pp. 357-73. The above citation is from Gardner's anthology, pp. 362-3.

²W. V. Quine, "Truth by Convention," in Otis H. Lee, ed., Philosophical Essays for A. N. Whitehead (Longmans, Green & Co., Inc., New York, 1936). It is reprinted with corrections in Paul Benacerraf and Hilary Putnam, eds., Philosophy of Mathematics (Prentice-Hall, Inc., Englewood

In other words, a term occurs essentially if its replacement in a statement may change the truth-value of that statement. For example, in the statement, "A cell is a cell," the term cell does not have an essential occurrence because any other grammatically admissible expression, replacing each of the occurrences of the term cell, preserves the truth-value true. But in the statement, "A cell is a mechanism," the term cell occurs essentially because its possible replacements may alter the truth-value; whereas the statement, "A cell is a mechanism" is, presumably, true, the statement, "A man is a mechanism," is possibly false and the statement, "A blueprint is a mechanism," is certainly false.

A term is specific to a discipline if and only if it is neither used (as a primitive term) nor defined in any preceding discipline. The term cell is specific to the science of biology but not to that of psychology (which presupposes the vocabulary of biology). To take another example, the term and does not have a specific occurrence in the statements of biology because it receives its

definition, or at least the rules for its use, in the presupposed discipline of logic.³

There is no doubt that historians use terms in an "essential way." The term Caesar occurs essentially in the statement, "Caesar crossed the Rubicon," because if we replace "Caesar" by any other personal name we may alter the truth-value. But the fact that a term occurs essentially does not justify the assumption that it occurs specifically. In the biological statement, "A cell is a mechanism," the term cell is used both essentially and specifically, whereas the term mechanism is used essentially but not specifically. Both terms are used essentially because the replacement of either term may change the truth-value of the statement. But the term mechanism is not used specifically because it is taken from the presupposed discipline of physics. On the other hand, the term cell receives its definition, or the rules for its use, within the science of biology itself. The statement, "A cell is a mechanism," is a biological statement because

³I have simplified White's account. For his discussion of the specific occurrence of a term see Gardiner, op. cit., pp. 361-3.

it contains the essential occurrence of a term specific to the science of biology. At the same time, we see--by an analysis of this biological statement--that biology presupposes physics (or some part of physical theory).

Do any terms, which may occur essentially in historical statements, have the property of being specific to the discipline of history? Professor White holds that the assertion, "Constantine defeated Maxentius at the Milvan Bridge on October 28, 312," is a specifically historical statement.⁴ This commits White to the proposition that this sentence contains at least one specifically historical term which occurs essentially. We should, therefore, be able to "decompose" the sentence and thus discover any specifically historical terms.

White begins the reduction by classifying "the Milvan Bridge" and "October 28, 312" as physical terms which, although essential to the expression of some historical facts, "are surely not specific to history."⁵ White, surprisingly enough, does not carry on the reduction.

⁴Gardiner, op. cit., p. 365.

⁵Ibid., p. 365.

If we attempt to pursue the reduction, or decomposition, we find ourselves confronted with a difficulty. At and on might be treated as relational terms pertaining to the sciences of measurement which explicate these terms. However, the term defeated poses a special problem. White has no suggestions to offer about where it belongs. We might say that the context of the concept of defeat as it is used in the assertion, "Constantine defeated Maxentius at the Milvan Bridge on October 28, 312," is provided by political and military history. This is true, but the meaning of this concept is not explicated in history.

If a child does not know the meaning of "defeated" in the sentence, "Constantine defeated Maxentius," we may tell him that "defeated" means "won over" or "beat." We might look in the dictionary to find synonyms for "defeat" that the child would understand but we would never refer to a textbook of historical method in a search for synonyms. This is simply because words like "defeat," and its verbal transformations, are not technical terms specific to history. Such words designate concepts which are "common property." I do not say that a social theorist cannot take over the term defeat and give necessary and sufficient

conditions for its use (thus turning it into a technical term).

We are left, by a process of elimination, with the terms Constantine and Maxentius. These terms, as the names of persons, are not historical concepts. Nor would it do to say that naming-terms are specific to history. Names come into history because their bearers take on historical importance. But the names do not belong to history. As names they are nondescriptive. They simply serve to "pick out" entities.

If we replace "Constantine" by the description, "The man who defeated Maxentius at the Milvan Bridge on October 28, 312," the description preserves the denotation of the proper name but has a connotation which the proper name lacks. When the man who was to become emperor was given the name "Constantine" there was no implication, in the act of naming him, that he would win a victory at the Milvan Bridge. Even if he had lost the day he would not have lost his name. Quine talks of translating the name "Pegasus" into a description. But I think we should note that "Pegasus" does not refer to an historical entity, but to a mythological one. We may say that whatever is

Pegasus has the property of being the winged horse captured by Bellerophon. Unless we change our mythology, Pegasus could not be anything else. We simply would not call anything Pegasus (within the context of the Greek myth) that did not have this property. But "Constantine" does not mean "The man who defeated Maxentius at the Milvan Bridge on October 28, 312," although it happens that Constantine did defeat Maxentius. Things are otherwise with "Pegasus." Pegasus does not just happen to be a winged horse or happen to have been captured by Bellerophon. "Pegasus" does not pick out an actual entity which may have done, or suffered, any number of things and have any number of properties. It really picks out a description and it may be plausible, as Quine supposes, to treat the name (or "quasi-name") as translatable by a description.⁶

It would seem that White is mistaken in supposing that "Constantine defeated Maxentius at the Milvan Bridge on October 28, 312" is a specifically historical statement; unless, of course, he uses "specifically" in a somewhat

⁶W. V. Quine, "On What There Is," in From a Logical Point of View (Harvard University Press, Cambridge, Mass., 2nd ed., rev., 1961), p. 7.

different sense from that which he introduced when he talked about what it meant for a term to occur specifically. I shall consider this latter possibility shortly.

White considers the role of various terms of common occurrence in history, such as "revolution," "depression," and "famine," and notes that all of these terms are employed in other disciplines; e.g., political science, economics, or sociology. Some terms may be defined in these disciplines; others are drawn from daily life and are not, therefore, part of a vocabulary specific to history.

We are tempted to draw the conclusion that White believes that there are no specifically historical terms. William Dray appears to draw this conclusion when he says that, according to White:

. . . history is the polar opposite of logic in that it presupposes all the other sciences, and has no special technical terms of its own; they are all borrowed.⁷

But it is not clear that White holds that all of the technical terms used by historians are "borrowed."

⁷William Dray, Laws and Explanation in History (Oxford University Press, Amen House, London, 1957), p. 7.

There is an ambiguity which was brought out in White's dispute with Patrick Gardiner. Gardiner had thought White held that "it must therefore be the characteristic of an historical explanation that there should occur within it specifically historical terms."⁸ This is, I think, precisely what White does hold--not only for "historical explanation" but also for "historical statement." Gardiner drew the inference that a specifically historical term must be one which is defined, or introduced as a primitive term, by the historian.

But White's reply to Gardiner indicated that there was a misunderstanding--for which I think Professor White was responsible:

My reply to Mr. Gardiner is as follows. First of all he appears to neglect my main point, which is conditional and to the effect that if there are any specifically historical terms, we cannot distinguish them sharply from sociological terms. I should say that the term "co-ordinated voluntary effort" in the example quoted by Gardiner from Trevelyan can count as a sociological term and also as a specifically historical term.⁹

⁸Patrick Gardiner, The Nature of Historical Explanation (Oxford University Press, Amen House, London, 1952), p. 63.

⁹White, in Gardiner, Theories of History, p. 371.

This rejoinder to Gardiner seems to go against White's own analysis of what counts as the specific occurrence of a term. It is natural to suppose that the term "co-ordinated voluntary effort" is borrowed by history from sociology. White states that "history presupposes all of the sciences that deal with human purposive behavior."¹⁰ Since sociology is a science answering to this description, it follows, given this train of White's thought, that history presupposes sociology; and "co-ordinated voluntary effort," since it is a sociological term (according to White), is borrowed from sociology by history.

Dray is faithful to White's own remarks when he draws the inference that all technical terms used in history are borrowed if--that is--White's analysis of the vocabulary of history is correct. But this fact makes White's attack on Gardiner puzzling, to say the least. How can "co-ordinated voluntary effort" be a specifically historical term if it is also a sociological term and history presupposes sociology?

I think we must say that White overstates his case

¹⁰Ibid., p. 368.

in asserting that history presupposes all of the sciences that deal with human purposive behavior. He inveighs against a sharp distinction between history and sociology; for White, history and sociology are associated disciplines which are on a continuum. On White's view, "history" is not, like "biology" or "physics," a fairly well-defined concept. We call a work "historical" if it deals with people and their actions over some more or less limited span of time. The boundaries between history and the social sciences are not distinct. As we find less detail --fewer names and dates--and more generalizations, we are more inclined to speak of a work as an instance of sociology or political science rather than as an historical monograph; but we cannot, and should not try to, draw a fine line.

A number of terms--probably most of the terms sociologists use--have been used before the advent of sociology, as a distinct research area, by historians. But, since, according to White, history and sociology are so close together, it really does not matter who first introduced these terms. They are either terms taken over from everyday life or technical terms--such as the termin-

ology of class structure--which are now common to both disciplines and specific to both disciplines.

The sentence, "Constantine defeated Maxentius at the Milvan Bridge on October 28, 312," is specifically historical because it gives the dated result of an interpersonal encounter and contains a social concept. I have said that the term defeat is neither a technical term specific to history nor a technical term at all. But this does not preclude us from saying that it is a shared term in which the question of presupposed disciplines does not arise. When a man in the street uses the term defeated, it would be odd to say that his vocabulary presupposes that of sociology. He may not--as I happen to know from personal experience--even have heard of sociology. When the political scientist uses the term defeated, it is unenlightening to say that he presupposes the common vocabulary of his languages; he is simply using it. In this respect, "defeat" is shared by the man in the street, the political scientist, the sociologist and the historian. The term presupposes no prior discipline in which it is first defined or given a use. The term is specific to history in the weak sense that its use does not presuppose the vocabulary of a prior

discipline. It is not specific to history in the strong sense that anyone other than the historian who uses the term must be borrowing it from history. Had White made a distinction between a strong sense and a weak sense of "specific," Gardiner would have been in a better position to deal with White's article.

As I interpret White, he holds that history lacks specific concepts in the sense that it has no unshared concepts. Its "specific" concepts are shared. Therefore, there can be no independent historical theory. White draws this conclusion on the basis of his rejection of the notion that there is a sharp distinction between history and sociology. When the historian generalizes--or subsumes a specific case under a generalization--he is doing sociology.¹¹

White is right to maintain that the vocabulary of history is at least as rich as the vocabulary of what he takes to be its presupposed disciplines. But what White maintains actually follows from the patent fact that histories are written in natural languages, that there are no

¹¹Gardiner, op. cit., p. 372.

limitations on the topics with which historians may deal, and that natural languages are capable of absorbing any technical term from any discipline.

White has not shown that the language of history is no richer than the languages of the sciences, natural and social, it may presuppose. It is apparent that, as a general practice, the historian does not employ terms with the idea in mind that they are adequately explicated or defined in some other discipline.

Some words, such as the word "cell" which the historian may use in writing the history of biological research, are recognized as having precise meanings which are in no way altered by their occurrence in an historical text. But the historian has no grounds for believing that the term revolution has either a precise definition in some allegedly presupposed discipline or that there are strict criteria for its employment. We can explain the term but we do not give a recursive definition of it; nor do we state necessary and sufficient conditions for its use (which would, if the conditions could be given, imply that the term was a technical one). Patrick Gardiner probably had this in mind when, in reference to White's paper, he

remarked:

It would be wearisome to repeat the thesis that specific scientific terminologies are a function of particular scientific systems, and that history presents us with no such system.¹²

White treats history as if it were the last member in the sequence of the sciences--the richest in concepts but the poorest in theory; in fact, quite without any theory of its own which can be distinguished from that of sociology. But a great many terms used by historians have no place in any technical, scientific vocabulary.

White's analysis of the vocabulary of history is meant to apply to the vocabulary of history as we actually find it. Thus it is not unfair to criticize him for overlooking the role that nontechnical concepts, belonging to no particular theory, play in historical writing. It is, after all, very possible that a great deal of historical "theory" is nothing more than a commonsense understanding of human actions; an understanding which is expressed by means of nontechnical concepts. It is true that a good many nontechnical concepts are quite satisfactory for

¹²Gardiner, The Nature of Historical Explanation, p. 63.

scientific purposes and--for reasons I shall discuss in the fourth chapter of this monograph--I think it would be a mistake to drive a wedge between history and the sciences on the grounds that the language of history is relatively nontechnical whereas that of the sciences is technical and, in the ideal case, presented in the guise of an axiomatized system. Moreover, it can at least be argued that the language of history can be made more precise--a process which may be encouraged by developments in the social sciences which, possibly, will permit a scientific understanding of human actions which will prove superior to a commonsense understanding of human actions.

But history as we have it is another matter. Even if we assigned such concepts as "revolution" to sociology, as White does, we would still be left with a great many other concepts about whose properties White has nothing to say. We have, for example, moral and aesthetic concepts which frequently occur in historical texts. These concepts may be eliminatable from history but they have not been eliminated. Our moral and aesthetic notions, as applied to human behavior, are used to appraise both the present and the past; "deceitful" or "crude," "cruel" or

"brash," are terms which fit into a theory only in the sense in which we may say that our appraisals of ourselves and other people reflect a general, unformulated, idea of how people ought to behave and what is, and what is not, appropriate behavior.

If history shares these concepts with commonsense, and surely it does, there is little point in saying that history has no theory proper to itself. History, insofar as it is commonsense applied to a study of past human actions, is quite unlike any applied science. It has a moral and aesthetic dimension--possibly an inarticulate moral and aesthetic theory--which these disciplines lack.

It may be said that this evaluative dimension, expressed in concepts which are, at once, nontechnical and evaluative, is something the historian could do without. In the following chapter I propose to consider this point of view at some length.

White's apparatus is not set up to deal with the moral and aesthetic dimensions of history. His analysis of the vocabulary of history simply omits whole segments of that vocabulary. This is a hardly surprising omission. If White had studied closely the moral and aesthetic

vocabulary employed by historians he would have had to admit that it is, to say the least, misleading to treat history as, in effect, applied sociology or an aspect of sociological research. It would do little good to trace the moral and aesthetic concepts employed by historians to "commonsense" because "commonsense" is not a science.

We cannot show that history is like an applied science (or is an applied science) by an analysis of the vocabulary of history. If we hope to show that history could become more scientific--more like an applied science--we must transcend actual historical practice and consider the possibilities of history. This White, in his analysis of the vocabulary of history, has not done.

CHAPTER III

HISTORICAL OBJECTIVITY

The sciences, both pure and applied, are usually thought of as being "value-free" both in the sense that the propositions belonging to these sciences do not reflect the moral and aesthetic evaluations of scientists and in the sense that the results obtained by these sciences are quite independent of any moral or aesthetic assessment which might serve to guide an inquiry.

In a very broad sense of "value-free" no science is "value-free" because every scientist, in selecting certain things for study, makes the decision that these things are worth studying, either for their own sake or because of their usefulness for some end. His selection of an inquiry may be guided by moral or aesthetic considerations; even, it would appear, by theological or metaphysical interests.¹

¹See William Dray, Philosophy of History (Prentice-Hall, Inc., Englewood Cliffs, N.J., 1964), p. 29.

But the inquiry itself is not supposed to be guided by any outlook, moral, aesthetic or religious, which yields results that only a man with the same kind of outlook can accept.

The scientist, most people would agree, is supposed to meet two requirements if his work is to be considered objective. First, his statements are supposed to be "value-free" and, second, his method of selecting material for an inquiry is supposed to be free of any value scheme which might make his claims acceptable only to those who shared the scheme.

From an empirical vantage point, it does not seem as if historians have ever committed themselves to meeting, or trying to meet, the first requirement. A few selections from the history of historical writing will provide evidence for the claim that historians make "value-infected" statements:

Proud conqueror of a servile nation, Nero proceeded to the capitol and paid his vows. Then he plunged into the wildest improprieties, which vestiges of respect for his mother had hitherto not indeed repressed,

but impeded.

Tacitus on Nero.²

Vespasian showed . . . great restraint in his dealings with Licinius Mucianus, a bumptious and immoral fellow who traded on his past services, in the matter of the Syrian legions, by treating him disrespectfully.

Suetonius on Vespasian.³

The religion in which he lived and died was that of the church of Rome, to which in his correspondence with Racine he professes himself a sincere adherent. That he was not scrupulously pious in some part of his life, is known by the many idle and indecent applications of sentences taken from the Scriptures; a mode of merriment which a good man dreads for its profaneness, and a witty man dreads for its easiness and vulgarity. But to whatever levities he has been betrayed, it does not appear that his principles were ever corrupted, or that he ever lost the belief of Revelation.

Johnson on Pope.⁴

²Tacitus, The Annals of Imperial Rome, Michael Grant, trans. (Penguin Books, Harmondsworth, Middlesex, 1956; the title on the front cover and spine reads: Tacitus on Imperial Rome), p. 309.

³Suetonius, The Twelve Caesars, Robert Graves, trans. (Penguin Books, Penguin Books, Inc., Baltimore, Md., 1957), p. 281.

⁴Samuel Johnson, Lives of the Poets, Vol. II (World's Classics Series, Oxford University Press, London, 1959), p. 303.

Alexander VI (as the new Pope wished to be called) possessed remarkable acumen, excellent counsel, marvelous powers of persuasion and incredible ability and application in all difficult enterprises; but these virtues were far outweighed by his vices: utterly obscene habits, neither sincerity nor shame nor truth nor faith nor religion, insatiable avarice, immoderate ambition, more than barbarous cruelty and a burning desire to advance his children in any possible way. Some of them--so that to execute his depraved design a depraved instrument should not be lacking--were in no way less abominable than their father.

Guicciardini on Alexander VI.⁵

In the course of the long struggle over neutrality Wilson is the key figure, not merely because of the central power of leadership he exercised but because he was, on this issue, a representative American and a good Progressive citizen . . .⁶

Hofstadter on Wilson.⁶

In principle and doctrine, Hitler was no more wicked than many other contemporary statesmen. In wicked acts he outdid them all.

Taylor on Hitler.⁷

⁵John R. Hale, ed., Guicciardini (The Great Histories, Washington Square Press, Inc., New York, 1964), p. 90.

⁶Richard Hofstadter, The Age of Reform (Vintage Books, A Division of Random House, New York, 3rd printing, 1961), p. 276.

⁷A. J. P. Taylor, The Origins of the Second World War (A Premier Book, Fawcett Publications, Inc., Greenwich, Conn., 1963), p. 72.

Professor E. H. Carr has endorsed the view that the historian ought not to pass moral judgments on individuals (he allows, however, that they ought to pass them on institutions) in the lecture series collected together under the title, What Is History?⁸ It may, therefore, be fitting to close this more or less random collection of examples with one from Professor Carr's writings (the example, it is only fair to say, predates the lecture series by over two decades):

The high thinking of the young romantics at Premukhino was conditioned only by emotional self-indulgence and mutual admiration.

Michael, true child of the romantic age, continued throughout his life to shun the common bread of hard reality and to chew the sweetmeats and spices of his own fancy.

Carr on Bakunin.⁹

I have chosen these examples, ranging from the Roman period to the present, to indicate that history has always had what I will call a "judgmental aspect." In the examples I have given it is perfectly clear where the

⁸E. H. Carr, What Is History? (Penguin Books, Harmondsworth, Middlesex, 1964), pp. 75-86.

⁹E. H. Carr, Michael Bakunin (Vintage Books, A Division of Random House, New York, 1961; first published in 1937 by MacMillan), p. 39.

historian stands. He registers his approval or disapproval of the behavior to which he calls attention whether he does this subtly or boldly. But it is not obvious that the judgmental aspect of history is incompatible with historical objectivity. There is a line we all draw--even if we do not all draw it at the same place--between malicious propaganda and historical method. To bring out more clearly what I have in mind by saying this I shall discuss the examples from Johnson and Guicciardini.

It is clear enough, even for one who knows nothing of Johnson's own attitude toward Christianity, that Johnson disapproves of Pope's "idle and indecent applications." He does take a stand; he does judge Pope. But are "idle" and "indecent" purely signs of disapprobation? Certainly "idle" suggests that Pope was wasting his powers or amusing himself with work the literary critic would not take seriously, and "indecent" gives us some idea of the nature of Pope's "idle" applications. The trivia in question must have been of a nature offensive to most Christians and sexual or scatological in nature. (I am assuming that the eighteenth century use of "indecent" was the same as the contemporary use of the term. This assumption would not

hold good for "witty" which, in the eighteenth century, was more likely to be used to refer to a man whose humor reflected intellectual power.)

Johnson is telling us something about Pope as well as something about himself. There are reasons for his evaluations and these are given simultaneously with the evaluations. This is to say that Johnson's language serves two purposes at once and it is not easy, assuming that it is possible, to separate out the normative and factual components of Johnson's assertions. Yet, to some degree, we know what would count for, or against, Johnson's assertions.

Guicciardini's remarks about Alexander VI may strike one as coming close to ranting; yet a portrait of Alexander, probably not an inaccurate one, filters through the expression of Guicciardini's indignation. We do have a description to the extent to which we know what would count against this portrayal; and, as a matter of fact, we do have a fair idea of what would count against it. We might, incredibly enough, find out that the Borgia had been made Pope against his will and wished to retire as soon as it was safe to do so. We might discover that he thought his

children worthless and wished to block their temporal advancement. Stretching the imagination, without deserting the realm of the logically possible, we can picture the discovery of surprising documents which would indicate that Alexander VI was a popular man.

That this will not happen does not alter the point that Guicciardini's portrait of Rodrigo Borgia is falsifiable. This does not mean that Guicciardini was not passing judgment on the moral worth of the Pope. But even in this most obvious case of the historian acting as judge, the presence of a descriptive component in the narrative is undeniable.

Any theory of history which would classify the remarks of Johnson and Guicciardini as purely subjective would be an erroneous theory of history. But there is a theory of history--more precisely, a theory of historical writing--which holds that it is quite impossible to effect a complete divorce between the normative, or judgmental, components of an historical assertion and the factual, or descriptive, components. I think we can intuit that there would be a difficulty in effecting such a divorce but, according to this theory, it can be shown to be impossible

to effect such a divorce and, at the same time, preserve history. This theory is represented by Sir Isaiah Berlin.

Berlin holds that the historian cannot, even if he would, step outside the confines of his native language; the limits of his language are the limits of his world and make it impossible for him to achieve the objectivity one associates with the natural sciences. The historian, in studying the behavior of human beings, deals with entities he must assume are capable of initiative; are, to however limited a degree, capable of controlling their own destinies and inviting praise or censure for what they do. Berlin holds the view--which cannot be analyzed in detail here¹⁰--that the thesis of determinism is incompatible with the practice of history. This thesis is, strictly speaking, unthinkable:

. . . all the discussions of historians about whether a given policy could or could not have been prevented, and what view should therefore be taken of the acts and characters of the actors, are intelligible only on the assumption of the reality

¹⁰See my master's thesis, "Determinism and the Language of History," on file at the Graduate Research Library of the University of California at Los Angeles, for a general discussion of Berlin's view.

of human choices. If determinism were a valid theory of human behaviour, these distinctions would be as inappropriate as the attribution of moral responsibility to the planetary system or the tissues of a living cell. These categories permeate all that we think and feel so pervasively and universally, that to think them away, and conceive what and how we should be thinking, feeling, and talking without them, or in the framework of their opposites, is psychologically well-nigh impossible--as impracticable as, let us say, to pretend that we live in a world in which space, time, or number in the normal sense no longer exist.¹¹

I do not here wish to say that determinism is necessarily false, only that we neither speak nor think as if it could be true, and that it is difficult, and perhaps impossible, to conceive what our picture of the world would be if we seriously believed it . . .¹²

We could not--even if we did, somehow, find a way of expressing the thesis--put it into practice. The historian, in particular, cannot--even if he should wish to--treat men as if their actions were neither praiseworthy nor blameworthy. If he abstains from direct praise or blame he will nonetheless insinuate in what ways an.

¹¹Isaiah Berlin, Historical Inevitability (Oxford University Press, London, 1954), p. 32.

¹²Ibid., p. 33.

historical agent is, or is not, at fault. According to Berlin, the historian cannot avoid implicit or explicit moral appraisals, whatever efforts he makes to be impartial, because the concepts he employs mirror the world as he and his fellow men experience it; this world--our world--is not one in which moral judgments and descriptive assertions can be sharply distinguished:

We account for the French Revolution or the character of Napoleon or the behaviour of Talleyrand as we would account for the behaviour of our own contemporaries and events in our own lives, public and private, with the same rich, scarcely analysable mixture of physiological and psychological, economic and biographical, aesthetic and ethical, causal and purposive concepts, which provide what we regard as normal and sufficient answers to our normal questions about how or why things and persons act as they do. All attempts to construct special sets of concepts and special techniques of history (e.g. by Marxists) have broken down because they proved sterile, for they either misdescribed--overschematized--our experience, or they were felt not to provide answers to our questions. . . . But just as our ordinary speech would become fantastically distorted by a conscious effort to eliminate from it some basic ingredient--say, everything remotely liable to convey value judgments, our normal, scarcely noticed, moral or psychological attitudes--and just as this is not regarded as indispensable for the preservation of what we look upon as a normal

modicum of objectivity, impartiality, and accuracy, so, for the same reason, no such radical remedy is needed for the preservation of a reasonable modicum of these qualities in the writing of history.

There is a sense in which the physicist can, to a large degree, speak with different voices, as a physicist, and as a human being; although even there the line between the two is anything but clear or absolute. It is possible that this may in some measure be true of economists or psychologists; it grows progressively less true as we leave mathematical methods behind us, for example, in palaeography, or the history of science or that of the woolen trade; and it comes perilously near an absurdity when demanded of social or political historians, however skilled in the appropriate techniques, however professional, however rigorous. History is not identical with imaginative literature, but it is certainly no less free from what, in a natural science, would be rightly condemned as unwarrantably subjective or personal. Except on the assumption that history must deal with human beings purely as objects in space--must, in short, be behaviourist--its method can scarcely be assimilated to the standards of an exact natural science. The invocation to historians to suppress even that minimal degree of moral or psychological evaluation which is necessarily involved in viewing human beings as creatures with purposes and motives (and not merely as causal factors in the procession of events), seems to me to rest upon a confusion of the aims and methods of the humane sciences with those of natural science. It is one of the greatest and

most destructive fallacies of the last hundred years.¹³

Berlin assumes, correctly, that there is a large class of historical sentences in which no effort is made to distinguish between value judgments and pure description. He assumes, more questionably, that for a wide range of cases of such sentences the attempt to make the distinction would produce a fantastic distortion of our language. Examples of such cases are the sentences, "Pasteur was a benefactor of mankind," "Hitler was an evil-doer," and "Lord Macaulay was less impartial than Ranke."¹⁴

The question I shall now raise is: is it possible to construct a non-evaluative class of sentences which would replace the triad of sentences I have given without (a) distorting our language or (b) failing to provide the same amount of information that the replaced sentences can be presumed to provide?

¹³Ibid., pp. 52-3.

¹⁴Ibid., p. 49. None of these sentences are verbatim examples from Berlin. I have lifted the first two out of their immediate context and put them into the declarative mode. The third is merely suggested by the text. It is the result of simplifying one of Berlin's examples.

By an "evaluative sentence" I mean one from which it is possible to infer (under a standard or "typical" interpretation of the sentence) whether or not the speaker, or writer, of the sentence morally approves, or disapproves, of the state of affairs the sentence is about. By a "non-evaluative sentence" I mean one from which it is impossible to make this inference (under a standard interpretation of the sentence). I shall not consider sentences which express aesthetic viewpoints, although these are also evaluative sentences and are of common occurrence in historical texts. My artificial use of the expression "evaluative sentence" is simply a convenience for present purposes.

Berlin claims that a history made up of non-evaluative sentences would prove unsatisfactory even if it could be realized. According to Berlin, an objective history would be purely behaviorist. At the linguistic level this means that any historical sentence--in a "scientific history"--would be expressed in a physical object language sufficiently enriched to express the concepts of behavioristic psychology. Such a language would, however, not be rich enough to express the concepts indispensable to the

historian. The simple sentence, "Jones robbed the Vine Street Bank at 4 P. M. on April 15, 1957," may be easy to verify but we cannot give a behavioristic analysis of the sentence. "Robbery" is a social concept which cannot be reduced to a description of the physical movements of Jones.

However, the series of physical movements executed by Jones is not thereby rendered unimportant nor is the concept of "robbery" rendered subjective simply because it cannot be defined within the framework of a behavioristic language. The sentence, "Jones robbed the bank," is just as objective as the sentences, "Jones ran from the bank" or "Jones had a temperature of 100.5" or "Jones weighed 203 pounds." Anyone who knows what robbery is knows how to verify or falsify the sentence, "Jones robbed the bank." Objectivity is not to be confused with measurability. If the historian writes "George I, the king of England, knew very little English," we must not suppose that the sentence is less objective than a statement of the population of England at the time of the reign of George I. We know how to discover whether or not a person has an adequate knowledge of English just as we know how to find out the

population of a country.

A good many sentences which incorporate non-behavioristic concepts are objective. We can get everyone to agree on some method of verifying, or falsifying, them. For example, the statement, "Truman voted for Dewey" would be--if true--an interesting historical statement but, despite the socio-political concept of "voting," we would not know, from the sentence itself, whether the historian approved or disapproved of Truman's action; but we would know how to go about verifying it. We may, following the terminology introduced by G. G. S. Murphy, call it a "quickly-decidable sentence" because it is one which, in theory, can be verified by anyone who has mastery of the concept incorporated in the sentence.¹⁵ "Truman voted for

¹⁵G. G. S. Murphy, "Sir Isaiah Berlin on the Concept of Scientific History: A Comment," History and Theory, Vol. 4, No. 2. I owe a great deal to this article but I use the notion of a "quickly-decidable sentence" to apply to any sentence whose truth-value we know how to establish. Murphy limits the class of quickly-decidable sentences to the class of atomic sentences (sentences consisting of constants and relations but lacking in the quantifiers "all" and "some") whose verification or falsification is, in theory, a simple matter. However, "Truman voted for Dewey" would be a quickly-decidable sentence for Murphy just as it is for me.

Dewey" would be a quickly-decidable sentence not because it would be easy to verify (Truman may never tell and, perhaps, his ballot has been destroyed) but because anyone who knew what voting was and who Truman and Dewey were would know how to go about verifying it. The psychological associations the sentence may have for various people can safely be ignored when one tries to verify it.

The sentence, "Truman voted for Dewey," is a quickly-decidable non-evaluative sentence. The sentence, "Pasteur was a benefactor of mankind," is not a non-evaluative sentence because of the occurrence of the concept "benefactor," which reveals that the speaker approves of Pasteur. Each member of Berlin's triad contains a relational term which, in conjunction with the constants, reveals the moral position of the speaker. ("Evil-doer" is, covertly, a binary relation.)

No member of the triad is quickly-decidable because such concepts as "benefactor" or "evil-doer" do not provide us with a clue toward any specific method of verification of the sentences in which these concepts occur. We may see why this is by considering an example from T. D. Weldon:

Now suppose that you are giving and not receiving a testimonial to Mr. Jones and that you say of him "He is intelligent, industrious, and trustworthy. He is also a competent athlete and can speak and read French and German." If in reply you are asked "Is he also a good man?" you are again in something of a quandary. Does the prospective employer want to be told that Jones is a regular churchgoer, that he is faithful to his wife, that he does not gamble and is not a Communist? Or what?¹⁶

"Benefactor" and "evil-doer" do not designate specific activities. I know what to look for if I am told that "Truman voted for Dewey," but I do not know what to look for if I am told Jones was a benefactor.

Being a benefactor is not like being a voter. I cannot point to some one thing and say: "That is what it is to be a 'benefactor.'" There are any number of things a man can do which may lead us to call him a benefactor. There is no direct logical relationship between the statement that a man is a benefactor and any other statement one might make about his actions. But certain sentences, expressing various facts, will usually stand in a support-

¹⁶T. D. Weldon, The Vocabulary of Politics (Penguin Books, Harmondsworth, Middlesex, 1953), p. 186.

ing relationship to a moral judgment. They are used to support it if it is challenged and if they are shown to be false the moral judgment is usually withdrawn. These factual sentences may be said to be "associated" with a moral judgment. The term "associated" calls our attention to the fact that the relationship between moral judgments and factual sentences is pragmatic rather than semantic or syntactic.

For example, the sentence, "Pasteur is a benefactor of mankind," will probably be associated with the sentence, "Pasteur discovered a vaccine which would prevent or minimize the danger of smallpox." The associated sentence is quickly-decidable and, of course, we know that it is true. (Actually the associated sentence rests on quickly-decidable sentences about the frequency of smallpox among the unvaccinated as contrasted with its incidence among the vaccinated. We can talk about it as "quickly-decidable" because we can now look up the results that verify it. But, strictly speaking, it is not a quickly-decidable sentence. It presupposes too much to qualify as one.)

The associated sentence is non-evaluative, in the restricted sense of "non-evaluative" we are employing,

since we cannot infer from its assertion the moral position of the speaker. It is highly likely that an historian who wrote "Pasteur was a benefactor of mankind" would associate his statement with some such sentence as "Pasteur discovered a vaccine which would prevent or minimize the danger of smallpox." However, no historian is likely to mean by "Pasteur was a benefactor of mankind" that Pasteur discovered a vaccine which would prevent or minimize the danger of smallpox. What Pasteur did is taken to justify the evaluative sentence. The first sentence, as it is typically used, is broader in its scope than the second. We cannot translate the first sentence by the second. We cannot, therefore, say that Berlin's evaluative sentence can simply be replaced by the "objective" associated sentence. It is true, as Murphy has pointed out in the article to which I have already referred, that a "value-free" history (in the form of a chronicle) could be written in a sequence of quickly-decidable (usually non-behavioristic) sentences and it might seem that we could produce a "value-free" history simply by the operation of detachment; i.e., collecting together the sentences used to justify a moral assertion and simply omitting the moral assertion.

But this is to abandon the scope of the sentence (which is both descriptive and normative) that is replaced. It does not meet Berlin's challenge.

What would preserve the general scope of the first sentence? An historian is likely to call an individual a "benefactor" of mankind if and only if that individual does something which has utility-value for some end human beings may be presumed to have. "Utility-value" is not the same thing as a moral value. It refers to a means-end relationship. Even if we should wish mankind to be erased from the face of the earth, we must agree that smallpox vaccine is one means to the end of sustaining human life for a longer period of time. The vaccine is useful for that purpose--it has a utility-value.

The sentence, "Pasteur did something which had utility-value for mankind," is verified by the fact that Pasteur discovered a smallpox vaccine which had utility-value. It will be noted that this sentence is general in scope. Any number of different actions by Pasteur would serve to support it or overthrow it. It is not an evaluative sentence because we cannot infer that the speaker or writer approves of Pasteur's action although that action

supports his utility-sentence. He may, for all we can tell, admire the spectacle of smallpox or have a hatred of mankind surpassing that of Swift.

Our replacement for the evaluative (or mixed evaluative/descriptive) sentence is not yet complete. When the historian writes, "Pasteur was a benefactor of mankind," we have the right to presume that he favors what Pasteur did and thinks that everyone else ought to admire Pasteur. It might seem that we could replace the sentence, "Pasteur was a benefactor of mankind," by the sentences, "Pasteur did something which had utility-value for mankind" and "I admire (or approve of) what Pasteur did and think that everybody else should admire (or approve of) what he did." The second member of the conjunction, as a statement of the historian's attitude, is an empirical proposition; it is a report of the historian's attitude and not an appraisal of Pasteur. The first member of the conjunction is subject to confirmation or infirmation by means of an assignment of truth-values to whatever verifiable sentences are associated with it.

However, it is not clear that the replacement preserves the cognitive significance of the sentence,

"Pasteur was a benefactor of mankind." It can be argued that the historian meant to say that he approved of Pasteur because he was a benefactor of mankind and Pasteur proved to be a benefactor of mankind because he made advancements in bacteriology which had utility-value for mankind. If the historian were writing a life of Pasteur his descriptions of what Pasteur did would serve both to justify his favorable attitude toward Pasteur and to explain his appraisal of Pasteur as a benefactor of mankind.

To treat the appraisal itself as "purely subjective" and, therefore, to drop it out of a proposed replacement for the sentence, "Pasteur was a benefactor of mankind," would be to beg the question of the role of appraisals in history. It is as if someone were to propose that critics of the arts abandon such terms as "awkward," "stylized," "delicate," or "mawkish" or, at least, agree that replacements which eliminate the element of appraisal would be quite acceptable translations which expunge, if anything, only a purely subjective tone which has no cognitive significance whatsoever.

If appraisals (or, at any rate, moral and aesthetic appraisals) are "purely subjective," this is something

which has to be shown. It is not enough to accept a doctrine and use this doctrine as the basis for a replacement thesis. The argument, I would say, is well taken. We certainly do not act as if our moral and aesthetic judgments were purely subjective. And it is a fact that when we wish to make it clear that our opinions are, or may be, purely subjective or reflect a purely personal point of view, we shift from second or third person sentential constructions ("You are being rude," "She dresses in poor taste") to a first person construction ("I feel he is being unfair," "Personally I find 'hard-edge' art less interesting than pop-art," or "I don't like, or approve of, Malamud's style").

It is always hazardous to treat such shifts in sentential structure as reflecting an underlying identity of meaning and implication. If I say that Malamud's A New Life is a badly written book, drawing on clichés of romance fiction to provide a novelistic background for a very interesting, sometimes humorous and sometimes--for the prospective academic--disheartening, study of life in a short-on-funds-and-brains state college, I shall not take kindly to the person who tells me: "It is only badly writ-

ten from your point of view; the clichés are only clichés in your judgment; one man's humor is another man's boredom --the same goes for 'disheartening'--and who is to tell what really is 'romance fiction' or what it really is to be short on brains? The only objective remarks you have made are those dealing with the name of the author, the title of the book, the fact that the setting is a state college and the fact that Malamud portrays the college as requiring money if it is to expand." If I had not been sure of my ground with Malamud's novel, I would have "personalized" my remarks. That is, after all, a standard retreat for one who finds himself at a loss for reasons to back up what may be very hasty judgments.

It is true, as everyone knows, that there is a great deal of disagreement about moral and aesthetics disputes and about the "logic" of moral and aesthetic language, or appraisal language in general. But it does not follow from this fact that, as a convenience for the purpose of "objectifying" history, we are entitled to treat the judgmental aspect of history as "purely subjective" and make it appear that our "replacements" for appraisal sentences really preserve all that is of cognitive

significance in the sentences which are replaced.

The argument that we cannot, so to speak, translate appraisals out of existence is a very powerful one. Even if we did eliminate appraisal terms, or mixed descriptive/evaluative sentences, from historical texts, it would not follow that we had eliminated appraisals from history. It has been argued that the historian, however "objective" his language, will select at least some of his material in accord with his notions of what is fitting or proper. If his appraisals do not appear in his text they will, nonetheless, be behind his text.

To show that a scientific history is possible we must demonstrate that appraisals--of any type which would not be classified as "objective" by the standards of the sciences--are not relevant to genuine historical inquiry. We must show that such appraisals, even if psychologically unavoidable, are theoretically unnecessary for historical understanding.

Whitehead, in his Adventures of Ideas, remarks:

The historian in his description of the past depends on his own judgment as to what constitutes the importance of human life. Even when he has rigorously confined himself to one selected aspect,

political or cultural, he still depends on some decision as to what constitutes the culmination of that phase of human experience and as to what constitutes its degradation. For example, considering the political history of mankind, Hegel saw in the Prussian state of his date its culmination: a generation later Macaulay saw that culmination in the English constitutional system of his date. The whole judgment on thoughts and actions depends upon such implicit presuppositions. You cannot consider wisdom or folly, progress or decadence, except in relation to some standard of judgment, some end in view. Such standards, such ends, when widely diffused, constitute the driving force of ideas in the history of mankind. They also guide the composition of historical narrative.¹⁷

If we decide that appraisals--moral or aesthetic--are purely subjective or can be "subtracted from" mixed evaluative/descriptive sentences without any loss in cognitive significance (and thus accept a theory of the subjectivity of moral and aesthetic judgments) we may find ourselves in the untenable position of demanding the elimination of history. If Whitehead is right, and we cannot get rid of moral and aesthetic presuppositions in

¹⁷A. N. Whitehead, Adventures of Ideas (A Mentor Book, The New American Library, New York, 1955; first published by MacMillan in 1933), p. 12.

history, our willingness to accept the belief that moral and aesthetic judgments have no cognitive significance may force us to characterize history as ultimately subjective.

Professor Dray has advanced an argument designed to show that historical inquiries into the causes of events are value-guided. Not only do historians pass moral judgments on individual persons but in assigning the causes of events they are guided by moral considerations. Actually, the distinction between passing a moral judgment on a person and being guided in one's causal analysis of past events by moral considerations is likely to break down in history. If I say that "Hitler was an evil-doer," I not only appraise and make it clear that I disapprove of the man but I also treat him as a causal agent. If my reason for my appraisal is, say, his racial policy, the enforcement of which led to the deaths of millions of people (I may have many reasons rather than just one for the moral judgment that Hitler was an evil-doer but if I had just one this would be it), I make it clear that I consider Hitler the cause of genocide. A man's personal life enters into social and political history insofar as his personal life is thought to provide a key to certain of his actions

which are judged to be of historical significance. (This is not a hard-and-fast rule, but I think it is a fairly accurate description of historical procedure.) An historian may, of course, pass moral judgments in cases where he does not think that the man's character led to actions of historical significance (he may just wish to give an example of a certain type of person to fill in a picture of an age) but, quite commonly, his moral appraisals will shade into, or be a part of, his causal analyses. A consideration of Dray's argument will make this point clearer.¹⁸

Dray bases his argument on an analysis of theories of Civil War causation. There is, as we know, no generally accepted theory of the primary cause of the American Civil War. Few historical events have been as much researched as the American Civil War and one might think that with the knowledge now at our disposal we could come to an "objective" decision about how the war actually came about and who, or what, caused it. But, it would appear, we are no

¹⁸Dray, Philosophy of History, pp. 47-58. These pages, with slight changes, incorporate the substance of an article, "Some Causal Accounts of the American Civil War," first published in Daedalus, Vol. 16, No. 3, pp. 578-92.

closer to an agreement about the primary or essential cause of the American Civil War than were the people who fought in it. We may think that we still lack sufficient information or--perhaps--will never have enough information to provide a really satisfactory account of the war and the causes of its outbreak. Dray, however, while not denying this possibility, thinks that the source of conflicting interpretations of the Civil War really has little, or nothing, to do with the amount of information at the historian's disposal. Even the earliest major theory of the American Civil War, "the conspiracy theory," can still find supporters and has not obviously been ruled out by the evidence.

Dray discusses three of the main currents of Civil War interpretation under the headings of the "conspiracy" theory, the "conflict" theory, and the "revisionist" theory. There are, today, "conspiracy theorists" who trace the cause of the coming of war to deliberate acts of provocation, "conflict theorists" who treat the coming of war as only to be expected given the differences between the North and the South, and the "revisionists" who deny that the outbreak of war was only to be expected (or even inevitable)

and, while rejecting the conspiracy theory, see the outbreak of war as the result of blunders, passion, and moral fanaticism which--at least in the case of the blunders--could somehow have been avoided. I shall first consider Dray's analysis of the conspiracy theory:

The conspiracy theory, as the name suggests, selects as cause the actions of certain individuals and groups--the "conspirators." It was a popular theory both during the war itself and during the reconstruction period, not only with avowed propagandists and sensation-seeking journalists, but with serious-minded (if self-taught) historians like Bancroft, Lunt, and Jefferson Davis.¹⁹

This theory which, as Dray remarks, "has made something of a comeback in recent years,"²⁰ always has an evaluative aspect:

When we ask who the conspirators were, however, the value judgment implicit in their selection immediately shows itself in a significant split along sectional lines. To southern historians they were northern abolitionists, out to eradicate the South's "peculiar institution" by the only method possible: the violation of constitutional guarantees after the capture of the federal government; or they were

¹⁹ Ibid., p. 48.

²⁰ Ibid., p. 47.

"Black Republican" politicians, determined to make use of the slavery issue to achieve control of the federal government. To northerners the villains were the planters and their political allies, determined to protect slavery by ensuring its spread to the territories, and perhaps ultimately to all the states. As the contemporary historian, George Bancroft, put it, "leading Southern politicians" had engaged "for over a quarter-century in a conspiracy to disrupt the government which they could no longer control." The time seemed to have come when it was necessary to determine by whom the Union was to be run.

Now what sort of a dispute is this? There are certainly important nonevaluative points at issue. The case of each side, for example, depends upon the correctness of the attribution of certain motives to the alleged conspirators of the rival section. But even if the sectional advocates reach agreement as to the motives and intentions of all the actors, the question remains whether it was the attempt, say, of the northerners to eradicate slavery that caused the war, or the attempt of the southerners to protect it. For the historians of the period, the choice between these possibilities rested on moral grounds: it was based on an appraisal of the reasonableness or degree of justification of the actions of each side; it depended on the answer given to the question: Who was being aggressive and who was simply defending his rights? To southerners, the secession of the South (even the firing on Sumter) was simply a warranted response to a northern threat and was not a cause of the war, even though it was a necessary step in bringing it about and may even have initiated it.

To northerners, denying the constitutional right of secession and claiming legal possession of federal property in the states, the cause was southern resistance to rightful occupation--the last act in a series expressing resistance to the idea of the Union. The concept of causation employed by such conspiracy theorists on either side is thus logically tied to the evaluation of those actions which are candidates for causal status. That the actions of either Black Republicans or the slaveholders and their allies were cause of the war is a judgment requiring the prior judgment that these same actions were reprehensible.²¹

The sentences, "The actions of the Black Republicans were the [primary] cause of the Civil War" and "The actions of the slaveholders and their allies were the [primary] cause of the Civil War" are seemingly incompatible and "undecidable." Dray allows that the sentence, "Lincoln was the cause of the Civil War," may be decidable on the basis of empirical evidence alone without any evaluative element playing a role in verifying or falsifying this sentence. It would be possible, at least in theory, to prove or rebut the contention that Lincoln personally plotted the war and arranged its outbreak.²² In

²¹Ibid., pp. 48-9.

²²Ibid., p. 50.

other words, Dray allows that some historical analyses of the cause of an event may be carried on without the element of moral assessment.

But if we allow that Lincoln's actions were not the primary cause of the coming of war we have to face the dilemma which the conspiracy theory represents. If the historians are to abstain from value judgments, it would seemingly be impossible for them to say that either the actions of the Black Republicans or the actions of the southern slaveholders and their allies were the primary cause of the Civil War. To single out the actions of one group or the other is to imply that those actions were deviant or "blameworthy."

The conspirators are the people who did what they should not have done. A "moral" counterfactual conditional is implied: had the conspirators been decent men they would not have conspired to bring about, or gamble on the outbreak of, civil war. If all historians agreed, say, that slavery was an evil to be eliminated at all costs, there would be no difficulty in assigning primary causes. The people who were determined to preserve slavery would be the ones responsible for the coming of the war because they

would be the ones who were behaving immorally and unreasonably. But, although most historians are prepared to condemn slavery, there is no universal agreement that slavery was a greater evil than civil war. In one way or another, historians will disagree with one another about who was behaving reasonably and who was not.

Dray considers other interpretations of the American Civil War and shows how the same kind of dilemma arises as that which comes out of the conspiracy theory. If we say that the situation brought about the war--that war was only to be expected because of the predicament in which both sides found themselves--judging "that no course of action that could reasonably have been expected from the men of either side would have succeeded in avoiding war,"²³ we commit ourselves to the propositions that it was not to be expected that the North would abandon the western territories to slavery or that the South would surrender to the Union government without a shot or take steps to limit, and eventually to eradicate, slavery as a sign of good faith while negotiating with the federal government.

²³Ibid., pp. 51-2.

We do have a notion of what we can expect of human beings and what circumstances exculpate them from a charge of moral dereliction. The theory of an irrepressible conflict is the obverse of the conspiracy theory. Blame is withheld rather than assigned. But the moral dimension of the theory remains an ineradicable aspect of it. If we think that men, on one side or the other, should have taken exceptional actions to meet a unique threat, we will not accept a theory which transfers the cause of the coming of war from agents to the environment in which they acted.

If we accept, or if we reject, the conflict theory, we do so--it would appear--on the basis of our moral conception of the human condition. If I point to all the facts the conflict theory has as its empirical base and produce any number of quickly-decidable sentences which I treat as justifying my appraisal of the war as a conflict that was only to be expected, I do not thereby prove that my appraisal is the correct one. For there are conflicting theories and, as things stand, no moral consensus which will allow us to rule out all theories but one.

Dray allows that the conflict theory, like the conspiracy theory, has an escape hatch for those who wish

to avoid moral judgments about the Civil War.³⁰ If the problem situation could be shown to be a sufficient condition for the outbreak of war--if the war could be known to be inevitable--then the sentence, "The primary (more strongly: sufficient) cause of the Civil War was the problem situation," would be a non-evaluative sentence. But we are hardly in a position to show that this sentence is true and it is not surprising that few historians, as Dray comments, are really willing to treat the war as inevitable.²⁴

The "revisionists" also present their material within the framework of an appraisal. They condemn statesmen for not coming up with satisfactory negotiations; they assume, at least implicitly, that the price of war was too high a price to pay for the eradication of slavery. They may--and are very likely to--admit that slavery was an evil but they maintain that war was a greater evil. From their perspective the people who sought the immediate eradication of slavery, at whatever price, could only be fanatics who should not have commanded the respect of those

²⁴Ibid., p. 52.

in charge of the federal government. By the same token, southerners who insisted upon secession rather than continued negotiations were at fault for valuing an intransigent stand above peace. They, and the followers they roused and were roused by, were also fanatics or blunderers.

These theories, and their offshoots and reformulations, obviously incorporate value judgments into causal judgments. Dray's study of theories of the American Civil War could, clearly, be matched by a study of the theories of the First World War, the French Revolution, or, I should think, any important upheaval. The judgmental aspect of history seems intrinsic, rather than accidental, to history.

I shall review what has been said so far. Berlin is mistaken in supposing that an objective chronicle, transcending a treatment of human beings "purely as objects in space" cannot be constructed. But chronicles fall short of history. A computer might order the data for a descriptive history--the data consisting of quickly-decidable sentences--and the resultant text might provide us with a great deal of knowledge about the past. (Admittedly, we

would have had to select the data to feed into the computer.) But the text would contain no explanations. No doubt, very often, we would fill in the text on the basis of our notions of what is likely to go with what. But we would have no warrant for doing this.

In practice, an historical text consists of facts, explanations, and appraisals of various sorts which it may be very difficult--if it is possible--to distinguish from one another. And even if we do sort out the facts, we are always left with the question of what these facts are supposed to show and why the historian has thought them, rather than others, worthy of mention. Very often the facts given serve as reasons for an appraisal. I justify the judgment that Churchill was a wise statesman by describing certain of his policies and the effect of these on his country.

If I gave just a list of his policies, no one would know what purpose the list was supposed to serve. A history which would go beyond chronicle and yet be "objective" in the sense in which the "hard" sciences are objective would have to link the facts by laws. Dray makes this point when he talks about the Hempelian account of

explanation. I shall discuss Hempel's account of historical explanation in a separate chapter, but here, in order to make the remarks, I shall quote from Dray understandable, I will simply say that Hempel's account of historical explanation rests on the contention that historians make use of laws in giving explanations. A satisfactory explanation will be one in which the facts are connected by laws.

Dray comments:

. . . if the Hempelian account of explanation is correct, there is no question as to the objectivity of the conclusions historians can draw. For the goal of explanation is to indicate sufficient conditions of what is being explained, and the sufficiency of these conditions will be certified by the fact that explananda will be deducible from them in accordance with empirically validated laws. There is just no room in such investigation for the value judgment of the historian. If there is room for disagreement at all, it could not legitimately have anything to do with the values of the historians concerned. It would be a result of the defectiveness of our knowledge of relevant laws.²⁵

Belin's arguments, drawn from the evaluative aspects of the language of history, are not sufficient for the purpose of drawing the conclusion that there could

²⁵Ibid., p. 23.

never be a scientific history. Dray admits that if the American Civil War could be shown to be inevitable "the element of value judgment" would disappear.²⁶ If I always could explain a man's actions (and only explain these actions) by means of laws, my moral judgments--from an historical standpoint--would not be important. These judgments, even if I were the sort of person who just had to make them, would form no intrinsic part of my historical research.

We may very well think, as Berlin and Dray do, that appraisals are an intrinsic part of building up an historical narrative which transcends chronicle. But we cannot eliminate the possibility of a scientific history unless we can show that not all historical explanations are, or need be, scientific; or, unless we can show that, aside from the evaluative aspects of the language of history, the language of history is, and must be, unsuitable for the purposes of a scientific history. In the following chapter I shall discuss the suitability of the language of history as an instrument for a (possible) scientific history.

²⁶Ibid., p. 52.

If moral and aesthetic appraisals are an ineradicable element of history, we cannot say, without qualification, that all historical statements are intersubjectively verifiable or that historical inquiries are "value-free." Strictly speaking, we can neither say that they are nor that they shall be. When we appraise something, we allow for disagreement of a type scientific methods can not settle. Perhaps the man who cannot see the merits of a Philip Guston painting is an "aesthetic oaf," and the art critic who gives Guston high marks and ignores the comments of the "oaf" is being quite objective. But the "oaf" does, after all, see the physical properties of the Guston as well as the critic. He misses, we may say, the aesthetic qualities to which the critic, by training and sensitivity, is receptive. It certainly would do no good to call in a specialist in the psychology of perception to talk things over with the "oaf." From a purely scientific standpoint, the intersubjectively verifiable sentences about the Guston painting are the ones even an "aesthetic oaf" could be trained to check out. We must admit that history as we actually have it is quite unlike any applied science from the standpoint of scientific criteria for

the classification of sentences (or "propositions" or "statements" etc.) as intersubjectively verifiable or falsifiable.

CHAPTER IV

NATURAL AND ARTIFICIAL LANGUAGES

Certain linguistic analysts believe themselves to be in possession of a distinction which, once its import is grasped, will settle the question of the scientific status of history. This distinction is that made between natural and artificial languages. In place of what may be called an intralinguistic analysis, in which features of the language of history--such as its vocabulary--are studied, we have an interlinguistic analysis in which the language of history is compared, in its structural features, with the language, or languages, of the exact sciences or mathematics.

"Analysis" is, perhaps, an overstatement, for it is not a question, here, of detailed comparisons between the language of history and that, say, of physics. There are no detailed studies in the philosophical literature

comparing specific explanatory forms in history and physics and eliciting similarities and differences between these. For example, one does not find an epistemological analysis of the similarities between functional laws in physics ("Volume times pressure is a constant") and historical statements like, say, "Hitler's power varied inversely with the ability of the Weimar Republic to prevent inflation."

Rather it is a question of a general view or way of looking at alleged differences between natural and artificial languages without the use of concrete examples. This view is to be found in the writings on history of Isaiah Berlin and rests upon the assumption--to be considered shortly--that the terminology of science gives us concepts with "sharp boundaries." Since the concepts of history, generally speaking, do not have "sharp boundaries" (i.e., neither the extension nor the intension of these terms specifies a clear-cut domain), and the propositions of the historian often "resist" classification into any sharply demarcated categories ("normative" or "descriptive"), it is supposed to follow that as soon as we grasp the distinction between the language of science (in the main, an artificial--mathematical--language) and that of

history we shall realize that history cannot become a science.

An argument quite like that of Berlin has been given a more-or-less canonical form by the philosopher, Christopher Blake, and I shall deal with his formulation of the argument in this chapter. The following quotation from Blake gives us the essential character of the argument:

Traditionally history was compared with the natural sciences either by subject-matter, wherein some said that they were alike in studying reality and some that they differed in being concerned respectively with men and with inanimate nature, or under the ambiguous notion of method. . . . However if we are to make a comparison that is both unambiguous and philosophically valuable we must abandon these traditional alternatives, and concentrate on a study of the languages which history and the sciences respectively employ. A study of the logic of the sciences--for example, of Physics, which is considered the most mature, and hence archetypal science--shows that their ideal structure is that of a formal deductive system. Such an artificial language is built around a logico-mathematical framework, and is consequently possessed of the important characteristics of: (a) conceptual precision; and (b) explicit rules to govern what can and cannot be said in the language and what does and does not contradict in it. . . . I emphasise that, although what we call the English language

is nowadays an amalgam of scientific and unscientific words, the large body of human discourse is and must be utterly lacking in those specialised and restrictive attributes. It possesses a logical structure infinitely more complex than that given in a formal system, and this complexity shows itself in so many directions that the concept of a "logic" of natural, or free, discourse is apt to mislead those who have the structure of formal systems in mind. Now I want to assert categorically that, while there remains an obsolescent tradition whereby history is called a science (c.f. the German word "Wissenschaft"), the language of the historian is, and again must be, of a natural, and therefore by comparison indeterminate, sort. The ideal scientific statement appears in a closed deductive system, while it is essential to historical writing that it enjoy the fluidity and adaptability of ordinary language -- "ordinary" not in the sense of "colloquial" or "plebian," but denoting a manner of stating and arguing that is common both to popular and to professional forms of speech.¹

This argument rests upon the undeniable premise that history is, and has been always, written in natural languages. These languages have grammars but do not have, either explicitly or implicitly, exhaustive "rules to

¹Christopher Blake, "Can History Be Objective?" Mind, Vol. 64. The citation is from the reprint in Patrick Gardiner, ed., Theories of History (The Free Press, Glencoe, Ill., 1959), pp. 335-6.

govern what can and cannot be said in the language and what does and does not contradict in it." Greater misunderstandings are possible between the users of these languages than is possible between those employing an artificial language (Blake does not stress this point but, surely, he would have to grant it); natural languages lack, in this regard, conceptual precision. We are led to suppose, from what Blake says, that a natural language is not suitable for the purposes of the most exact sciences and that no discipline which employs a natural language can be very close to the scientific deal.

Blake supposes that a natural language constitutes some kind of ideal structure (this "ideal structure" being, to put the point paradoxically, its lack of structure) which separates out its major features from those of any possible artificial language. One of these features is, presumably, the empirical character of grammatical rules. The rules of English grammar are not, at least for the most part, the result of legislation. Present day English grammar can be described as the history of sentential structures which have won out.



But what has "won out" can take on all the force of a formation or transformation rule in an artificial language. For example, in English, a pronoun used as an indirect object cannot precede its preposition. There is no dialect of English in which one would be allowed to say "He gave it me to" without correction. Empirical regularities have taken on the force of a formation rule in an artificial language and the fact that the formation rule, in this instance, is a result of wide-spread frequency in usage no longer matters. The student who writes, "He gave it me to" would be marked down for the same reason as one who writes "P Q V" in place of "P V Q." I do not wish to press this particular example but the very existence of such examples should lead us to be cautious in discussing alleged differences between natural and artificial languages. In any event, the syntax of English is adequate for many scientific purposes even if the "syntax" or "grammar" is--as is true enough--the result of frequency distributions of usage in the English language.

We see this as soon as we examine scientific practice. Blake talks as if the historian can be distinguished from the "hard" scientist in virtue of the fact that the

historian writes in a language which every educated member of his culture can understand, whereas the "hard" scientist writes in a language which requires highly specialized training for its mastery.

Precision, for the biologist, is not a function of the degree to which specifically biological terms are "encapsulated" in sentences constructed in an artificial language according to meta-linguistic rules governing that language. This precision is the result of agreements (or a tacit agreement) among biologists to use the same words in the same way. Although it is much easier in biology than it is in history to come to such an agreement there is nothing, in principle, which would prohibit historians from reaching agreements about how various words were to be employed in the future--or what new terms were to be introduced to meet the needs of historical research.

Even, to use Blake's preferred example of an exact science, the language of physics is not so rigorously ordered as Blake imagines. Some of the results of research in physics are expressed in English within the original papers giving those results. This is an obvious point but it has a not quite so obvious implication. The artificial

language--some possible axiomatized segment of mathematics-- is not equipped to deal with all of the statements the physicist wishes to make in his professional capacity. Physico-mathematical statements, in turn, require interpretation or exposition if they are of a fundamental nature. For example, the formula of the Lorenz contraction (according to which every moving object contracts in the direction of motion) is expressed mathematically as contracting in the direction of the motion in the ratio of

$$\sqrt{1 - \frac{u^2}{c^2}} \text{ to } 1.$$

The formula is intelligible only when one knows that "u" equals the speed of the object and "c" equals the speed of the propagation of light. But there is no precise definition of "light" nor is there--as yet--a universally acceptable theory about the way light is propagated. (Presumably, an acceptable theory about the latter would be treated as an acceptable definition of the former; i.e., "light" would be taken to mean particles or waves or quanta of a certain sort.)

The mathematical formula--once the physical constants in the formula are seen to occur both essentially

and specifically--receives its meaning within the context of that part of the natural language which has been taken over by the physicist. There is no purely mathematical understanding in physics although there are purely mathematical derivations. A theoretical physicist cannot avoid a natural language unless he is simply deducing consequences from a theory which has been explained elsewhere. To quite an extent the situation is similar to that obtaining in biology.

The physicist does work with a highly "precized" vocabulary (one in which ambiguities are at a minimum; I have borrowed the term "precized" from Professor Herman Tennessen). His use of "matter," "force," and "energy"--to take a common triad--are highly "precized" extensions of the meaning these words have had in typical discourse in the English language. (Some philosophers, such as Rudolf Carnap, would speak of scientific "explications" but the notion of "explicating" a term taken over from a natural language poses problems which it is not necessary to take up here.)

The "conceptual precision" of which Blake speaks is--as the example of the Lorenz Contraction suggests--

really a matter of a degree of "precization." As it happens, it is not necessary to settle the dispute about the nature of "light" in order to make use of the Lorenz formula.

The language of law is often viewed as an ossified form of English but, although there is some truth in this, this language, in the course of its history, has exhibited the continuous "precization" of the English language for legal purposes. What began as a notoriously vague terminology has become more and more precise.

This language (and, however horrible, it is English) deals, as much as history, with human actions and yet has achieved a high degree of precision and, in many areas of its application, has been, or is being, pruned of the moral overtones which one would suppose to be (following , for example, Berlin's argument) ineradicable.

Thus, from examples such as those given in the above, we see that the initial plausibility of distinguishing between history and the exact sciences on the basis of the allegedly different languages employed breaks down. There is no need to pursue these examples to bring out the questionable character of Blake's remark that the language

of history must be of an indeterminate sort in comparison with the language of science. Why must it?

The language of science is an extension of natural language. The concepts of science are also fuzzy at the edges; they have, so to speak, a life of their own and implications, when they occur in scientific propositions, which the scientist using them may not see. For example, Einstein's definition of "simultaneity" had consequences for a wide range of propositions in which the term "simultaneity" occurs. It is not to be supposed that, in 1905, Einstein had in mind every possible effect of his definition. No doubt his definition drew upon the way the term "simultaneity" had previously been used in physics, or elsewhere, but it would not be easy to say just where his modification came in and what of the older "definition-in-use" he had left alone. If we were able to accept Blake's picture of procedures in physics, we would be able to give a partial recursive definition of Einstein's concept of "simultaneity."

We could list the set of primitive terms common to Einstein's definition of "simultaneity" and the typical "definition-in-use." The primitive terms not shared in

common would give us Einstein's point of departure. That, as I believe, we cannot do this suggests that "simultaneity" was not a term which had clear boundaries in pre-Einsteinian physics. In fact, it is not even clear that Einstein did depart from previous usage. It could be argued that he simply drew out a very surprising implication of that usage which no one had ever seen before.

The language of law approaches, at times, the precision of certain aspects of physics. The language of physics, at times, approaches the "richness" or "ambiguity" of a natural language. Precision and looseness in language can be found both in daily life and in science. It is hard to understand why a comparison of the structure of the languages of science with the structure of "ordinary language" is supposed to reveal that the historians will never do what the biologists have done: will not, that is, revise their language. It is particularly hard to understand this when we consider that most sciences have the structure of the natural languages as their linguistic structure and even physics uses mathematics as an adjunct to its linguistic structure.

Presumably, Blake is not making a prediction. He

is not saying that he thinks the language of history will continue to be of such-and-such a kind or that historians will not go to all the trouble of revising it. The "prediction" would not be worth making; there is no telling about how much trouble future historians will take with their language.

Surely he does not think that he is making a recommendation of the "leave your language alone" type; he could not claim, on the basis of a recommendation, to be getting at the essence of history. Historians would be free to reject a recommendation; they might be foolish to do so, but that is quite another matter.

Obviously, he is making the very strong claim that the language of history does not admit of the degree of "precization" which would permit the historian to become a scientist and will never admit of it; the historian is wedded to an indeterminate language for all eternity. This conception of Blake's arises on the basis of a notion of the rigidity of natural languages. "Rigidity" is an odd word to use in this connection because Blake would be among the first to insist upon the flexibility of a natural language, but it is justifiably used.

It is apparent that he pictures "ordinary language" or "natural language" as an instrument for human communication whose value is to be found in its lack of precision. Anyone who "precizes" it by that same gesture destroys it. It loses in richness what it gains in precision. The damage is fatal. In other words, Blake has an a priori conception of a natural language; it is true by definition that "natural languages" are indeterminate to a degree to which no language used by the ideal scientist--the physicist--can be indeterminate. If we grant that histories will always be written in natural languages it will follow, by definition, that history is not a science. And, Blake must be given this, we find it difficult to imagine what it would be like for histories to be written in the language of mathematics. Yet even this difficulty can be exaggerated. Isaiah Berlin remarks that the historian uses "inevitable," "possible," and "probable" as these are used in ordinary nontechnical thought and speech.²

He implies that a mathematical usage would be out

²Isaiah Berlin, Historical Inevitability (Oxford University Press, London, 1954), p. 51.

of the question for the historian. The terms--under a mathematical interpretation--would no longer be useful. I take it that this is supposed to be so because the historian is not in a position to assign numerical values to probability statements (Berlin does not say just what he does have in mind). But it is quite easy--using the suggestions of Von Neumann and Morgenstern--to assign a non-numerical probability interpretation to these terms which, so far as I can see, does not the slightest damage.

"Inevitable" can be assigned the probability value, "near or at the limit 1" (this would preserve the vagueness Berlin insists upon); "possible" assigned the probability value, "greater than zero-probability"; and "probable" assigned the probability value, "within the upper half of the interval between 1 and zero probability." There is no reason to suppose that this interpretation distorts the historian's usage of these terms. In fact, if the interpretation does distort the usage there must be something wrong with the usage.

This interpretation looks very trivial but it has its usefulness. As soon as we assign this interpretation (and, contra-Berlin, I think we do this automatically), we

are able to bring in the notion of consistency in inter-relating the propositions of the historian. For example, under this interpretation: if the historian says "X was more probable than y and y was more probable than z," in one place, but goes on--fifty pages later--to remark that "Z was more probable than x," we can convict him of inconsistency on purely formal grounds.

He would be failing to make the right inference from his probabilistic propositions. I say on "purely formal grounds" because we need not know the numerical assignments in order to see that he has gone astray. Now I have no doubt that Berlin would also convict the historian of inconsistency in a case like this, but I do not see what grounds Berlin would have for doing so; unless, of course, he used implicitly the interpretation I have spelt out.

I do not, however, wish to deny the difficulty Blake raises or give that impression by my exposure of one of Berlin's rather careless remarks. I do not suggest that the language of history will ever be a mathematical language or that much of history will ever be expressed in mathematical formulae. From a theoretical perspective, I cannot really imagine what it would mean for the language

of any science to be mathematical. " $E = MC^2$ " is, possibly, the most famous formula in contemporary physics but, obviously, a knowledge of the language of mathematics would not permit one to understand it. "E," "M," and "C" are shorthand for concepts which have nothing to do with mathematics. But I will not further belabor the point; nor will I deny that contemporary physics could not exist without contemporary mathematics.

Furthermore, I admit that the association between physics and mathematics is far closer than I would ever imagine any association between history and mathematics could be. But this admission really concedes very little to Blake. The point is this. A language may be very precise and still not yield mathematical formula. A meta-language for mathematics is obviously of this type.

So is the language of botany or biology as I have already mentioned. It is true that meta-mathematicians, botanists, and biologists are, in their researches, free from the judgmental aspect of present and past historical research. But Blake's argument is a structural argument; although his article deals with questions of historical objectivity, the portion I have been concerned with is

supposed to develop an argument which would show that history is not a science whatever the role of appraisals in history; whether moral and aesthetic judgments could or should be abandoned.

To summarize the discussion: the distinction between natural and artificial languages does not match the distinction between the "hard" sciences and the humanities; there is no one-to-one correspondence. Natural languages do not possess some ideal structure of necessary vagueness which prohibits alterations; in point of fact, "areas" or "portions" of any natural language may be made more and more precise until one reaches the stage at which the distinction between a natural and an artificial language is difficult, if not impossible, to apply.³ Working in the reverse direction we can say that it is difficult, if not impossible, to point to any scientific concept which has achieved an ideal "precization" or any "language of science" bound, in all particulars, to the inexorable rules of a scientific meta-language.

³I owe this point, as well as much of the outlook expressed in this chapter, to Professor Herman Tennessen.

Whether or not the language of history can be made more precise--assuming that it is not already precise enough by scientific standards--is something that cannot be foretold from the structure of that language. We have in the previous chapter considered the point that if historians could link their data with laws the evaluative dimension of history would not be an intrinsic part of the discipline of history.

Similarly, before we can say that the language of history--whether or not evaluative--must be, by scientific standards, imprecise we must first consider the way in which historians do (or may in future) link their data. If historians do, or could, give satisfactory explanations of the past by means of well-established laws, the conceptual imprecision to be found in history would be no more, or less, significant than the conceptual imprecision to be found in the "hard" sciences.

From what has been said above, and in the preceding chapter, it can be seen why so many philosophers of history have been concerned with historical narrative in terms of its "story-telling" and explanatory texture. We can, as it seems to me, fairly easily show that history is not very

close to any applied sciences with respect to actual historical practice. The role of value judgments in history suffices to make this point. But there is a nagging doubt about the future of history. Suppose the historian wanted to be a scientist? Could he be one if he tried? Could he ever become one? When we raise these questions we are forced beyond the arguments that have been considered so far and must turn to discussions of the texture of historical narrative and the nature of historical explanation.

PART TWO

CHAPTER V

HISTORICAL NARRATIVE

In a recent book, W. B. Gallie has provided an analysis of historical narrative. He holds that historical narratives are stories the historian tells with the understanding that they be true stories. "History" means "a wide family or syndrome of researches and writings, the key members of which always contain narratives of past human actions. These narratives are followable or intelligible in the same general way that all stories are."¹

We understand what "historical narrative" is when we understand what it is to follow a (true) story. The historian, like the novelist, only succeeds in telling a

¹W. B. Gallie, Philosophy and the Historical Understanding (Chatto and Windus, London, 1964), p. 71. The analysis of historical narrative is given on pp. 11-104, inclusive.

story when he gives us a narrative in which we are able to see the interconnections between the events the historian deals with and understand the behavior of the participants in these events. Like the novelist, the historian deals with some passage of time in which there is a movement of human interest--activities on the part of human agents which bring about, and account for the complications of, the event in question.

On this view, Gibbon's Decline and Fall of the Roman Empire and Thomas Mann's Death in Venice are both stories. Neither Gibbon nor Mann give us a mass of "quickly-decidable" sentences. Gibbon leads up to the collapse of the Roman empire just as Mann leads up to the death of Gustave Aschenbach. In The Decline and Fall of the Roman Empire and in Death in Venice there is movement--toward decay--of human interest. Gibbon openly, and Mann more deviously, analyze this process of decay. What was it that brought Rome down? What was it that permitted, or even compelled, Aschenbach to abandon a safe and successful way of life in Germany and "accept"--I can think of no better word--a Venice ridden with plague and death? Gibbon and Mann trace a decline by reference to the activities of

human agents; to agents who "put themselves in a fix" or are simply trapped by circumstances but whose activities hold our interest and make us wonder, "What next?" What is a story? It is the sort of thing The Decline and Fall of the Roman Empire or Death in Venice is. It is a narrative of interconnected events which has a culmination or plot and in which, subtly or crudely, the behavior of the human agent involved in these events "makes sense." There is little point in seeking a definition of "story" because "story" is hardly a technical term, or one with a denotation and connotation which is beyond dispute. But we may pursue such notions as "culmination" or "makes sense" which will give us more of a feel for what it is for something to be a story and why it is so natural to describe a good deal of historical writing as "story-telling."

No more than the novelist does the historian simply take some segment of time and produce a "history" of what went on during that time. Many historians choose a subject which has, more or less, some upshot which they propose to illuminate by their investigations. For example, the historian may choose as a subject the diplomatic relations between Germany and Poland in the years 1938-1939. The

subject has an upshot--namely, the invasion of Poland by Germany and, shortly thereafter, formal declarations of war on Germany by France and Great Britain. The subject also has a cast of characters whose actions in those crucial years determined whether there would be a conflict in 1939; Beck of Poland, Hitler, and so on.

It is true that there are histories, primarily descriptive in nature, of the sort William Dray calls "period" histories, whose subjects are "determined chiefly by spatio-temporal criteria."² The "period" historian will not simply give (even if he could) the list of events which, in their totality, constitute the activities of the period about which he writes. On the other hand, since he is committed only to giving us a picture of some segment of the past, his inquiry is not necessarily guided by what he takes to be the culmination of some past events. He need not--although he may--tighten up his narrative by fixing on the interrelations of a small "cast of characters."

Gallie gives a "commonsense level" definition-in-

²William Dray, Philosophy of History (Prentice-Hall, Inc., Englewood Cliffs, N.J., 1964), p. 34.

use of "story" by listing the properties of the class of narratives connoted by the term:

Every story describes a sequence of actions and experiences of a number of people, real or imaginary. These people are usually presented in some characteristic human situation, and are then shown either changing it or reacting to changes which affect that situation from outside. As these changes and the character's reactions to them accumulate, they commonly reveal hitherto hidden aspects of the original situation and of the characters: they also give rise to a predicament, calling for urgent thought and action from one or more of the main characters. The predicament is usually sustained and developed in various ways that bring out its significance for the main characters. Whether or not the main characters respond successfully to the predicament, their response to it, and the effects of their response upon the other people concerned, brings the story to within sight of its conclusion.³

Gallie's notion of a "story" (a notion or, more strictly, use of the term which I find congenial) is that which we associate with literary criticism. With this use in mind, we may say that "period" histories are not stories. But, as I have already said, "story" is not a technical term. One loose definition of "story" is "the

³Gallie, op. cit., p. 22.

telling of an event or events." This definition fits any kind of history, explanatory or descriptive and, therefore, under this definition "period" histories are a form of story-telling.

Gallie, however, is prone to treat all histories as being stories in the literary sense, or use, of the term. He recognizes that there are histories which do not seem to possess the unity of structure one finds in most stories. About this fact he is tempted to say two quite different things and he succumbs to both temptations. First, he is tempted to say, echoing Ranke, that not all "histories" are really history. He recognizes that giving in to this temptation--without qualification--will give him an empty "victory." If he says that every "history" which is not an organized story (on Gallie's understanding of what stories are) is not really history, he may find himself making his analysis vacuously true by the automatic preclusion of possible counter-instances to the analysis. He does think, it seems to me, that some "histories" are not really history but--in an effort to avoid making his analysis vacuously true--presents the argument that other histories which may not seem to be genuine histories actually are

genuine histories because (in a non-obvious way) they do exhibit the unities we associate with a short story or novel.⁴ But it is obviously dangerous to insist that works satisfy the criteria that an analysis provides. There is the danger of "forcing" the material into the scheme provided by the analysis.

If a "period" history deals with subjects "determined chiefly by spatio-temporal criteria," Gallie must say either that this kind of "history" is not really history or that it is a history because "spatio-temporal criteria" actually provide sufficient unity for the purpose of telling a story. If he chooses the second alternative he, in effect, goes back to a non-literary definition of "story" and his analysis, which depends for its force and provocativeness on the literary use of the term, is actually abandoned. Not even James Joyce's Ulysses, which observes the unities of space and time of the classical drama, is a "period" novel although it is that very different thing, the novel of a period. There are very few literary works which are like--in their structure--"period" histories. There would

⁴Ibid., pp. 68-71.

be little point in saying that "period" histories are like (literary) stories because some (literary) stories are like "period" histories. This, even if true, would "dilute" Gallie's analysis.

It seems better to say that much of historical writing is "story-telling" rather than that all genuine historical narrative falls under the literary category of a "story." At least I shall assume that not all histories fit into the category of a "story" although I also assume, as has been made clear in previous remarks, that no historian simply "cuts off" a space-time segment and writes it up.

I do think, however, that every explanatory historical narrative may well be viewed as telling a story. In what follows I shall pursue a course of analysis--suggested by Gallie's approach--and continue to draw comparisons between the historian and the novelist, rather than introduce such qualifications as "the historian who constructs an explanatory narrative" or "a good many historians."

The historian seeks to illuminate the motives of his characters--show just how their actions make sense and

fit together to produce the result or results which terminate the core of his history. He makes the characters themselves intelligible to the reader; gives evidence which classifies these characters and suggests the possible range of behavior one might expect from each of them in the light of this information.

The historian's story goes from event to event with the end, foreknown, in mind. This is, again, in theory, what the novelist does; at least by the time he is ready to publish his book he is supposed to have produced a coherent manuscript in which the end is related to the beginning.

In both the case of the novelist and that of the historian, the events centered upon are shown to develop as they do because, at least partially, of the behavior of "members of the cast." Moreover, both the novelist and the historian are often content to show how the behavior of their "characters" is understandable rather than inevitable. The historian is similar to the literary artist in displaying motives, taking account of contingencies, freak happenings, and writing a coherent story of some aspect of the past.

This historian does not show that all "contingen-

cies" are seeming-contingencies, that all human actions are predictable in principle. He need not carry that burden if he is to convince us that the past is, to a large extent, intelligible. Few historians put forward the claim that the past had to be what it was. Beck might have reached an agreement with Hitler, but did not; history takes shape, in part, from the decisions men make about the emergencies confronting them. The historian does not always go behind these decisions to find applicable laws; we are usually content if he fits the decisions into the fabric of his narrative.

It may be, of course, that historians give us only partial explanations of the past to the extent to which they fail to demonstrate the inevitability of the actions, and events, they describe. This "may be" is the crux of the debate about the scientific status of history as an "explanation-providing" discipline. For if it is required that an explanation of some past action demonstrate the historical inevitability of that action, it is quite possible that there has never been an historical explanation of anything. If this be the case, one is confronted with the choice between denying that history has any scientific

status or asserting that historical explanations are always incomplete but approximate a scientific ideal to the extent to which they eliminate alternatives. This question will be taken up in the next chapter.

The novelist resembles the historian in his concern with contingency; unless he is writing from a thesis and producing those carbon-copy characters which are the stock in trade of the bad novelist, the author of "pot-boilers," he creates individuals of whom one has the feeling that they may react to their environment in more than one way; that they have creative potential and may well surprise the reader with some of the things they do.

Consider the behavior of Kavelerov in Yurii Ole-sha's novel, Envy. Kavelerov lives with a man he despises, Andrei Babichev, who has quite literally picked him out of the gutter. One decision Kavelerov has to face is that of whether he should hide his feelings to maintain his position for as long as possible in the Babichev household or tell Babichev what he thinks of him and declare his independence. From one moment to the next the reader is not sure of what Kavelerov will do. The point is that whatever Kavelerov does will be understandable to the reader because

either decision on Kavelerov's part makes sense; he has grounds for both.⁵

Whatever characters do, in the most tightly knit story, has a point; but the novelist--at least, the contemporary novelist--is not likely to lead one to believe that only what in fact they did do had a point; that there was one and only one path which was possible for them and he simply set them in motion along that path. One course of action may be just as "followable" as another. So long as the novelist gives us a followable or understandable sequence of events, he has fulfilled his obligations without necessarily claiming or denying that more than one sequence of actions in his story would be intelligible to the reader.

It is easy to find similar examples in history. The American historian of art, Sam Hunter, took up the question of what accounted for the post-war emergence of the "New York" school of painting. Until after the Second World War the name "New York" meant virtually nothing to

⁵Yurii Olesha, Envy, in the selection, The Wayward Comrade and the Commissars, translated by Andrew MacAndrew (Signet, The New American Library, New York, 1960).

the world of art (except as the name of a place to move to or sell paintings in). Why is it now possible to mention New York in the same breath with Paris when it is a question of contemporary painting? Hunter gives this account:

There were many factors that contributed to the renaissance of a vital advanced art, which took place in America during the war. Not the least of them was the concentration of expatriate genius in New York as a direct result of the international conflict. After 1942, Peggy Guggenheim's Art of This Century gallery became the first exhibiting center for the young American vanguard, and for a large group of European abstractionists and Surrealists, many of whom had taken refuge in America. Miss Guggenheim not only gave Jackson Pollock, Robert Motherwell, William Baziot, Hans Hoffman, Mark Rothko, and Clyfford Still their first New York one-man shows, but helped stimulate the exchange of ideas between the emerging young Americans and many of the more impressive reputations of Paris. Some of the Europeans who found themselves in New York at this time and directly affected American painting, ideologically as well as pictorially, were Matta, Tanguy, Ernst, Leger, Mondrian, and Masson. The migration of European intellectuals and artists to these shores renewed vital contacts that had lain moribund for many years, and helped free American artists of their provincial standards.⁶

⁶Sam Hunter, "The United States," Art Since 1945 (Washington Square Press, Inc., New York, 1962), p. 272.

Hunter's account, by ordinary standards, is intelligible or "followable." But we would be misreading Hunter if we thought he was saying that a renaissance had to take place because of the facts he lists--or even because of factors which he omits to list or has not thought about. After all, the "stimulation" may have come to nothing. Guggenheim and the established artists set the stage for the renaissance but they did not actually make it. The American artists did that. The Europeans gave them ideas and Guggenheim gave them a place to exhibit. But what they made of these ideas--what they actually produced for exposition at Art of This Century--is not so easily explicable.

Hunter foreswears any effort to show the necessity of this renaissance. This is made very clear when he discusses the paintings of one of the leaders of the New York school, Mark Rothko. Of Rothko he remarks:

The direction Rothko's painting was to take is foreshadowed by elements in his very earliest painting, although there was then no way of anticipating the unprecedented pictorial means he finally created.⁷

⁷Ibid., p. 306.

This remark can be paralleled by others Hunter makes about Pollock and de Kooning. In other words, we can detect certain of the "factors" which set the stage for the renascence and we can get a rough idea, in terms of "influences" and specific encounters, of why Rothko, Pollock, and de Kooning painted as they did. But Hunter does not present us with a set of developments which show that the renascence was inevitable. Perhaps we should say that he gives us a partial explanation, but if we say that we must be sure we have clearly in mind what we would mean, in such a context, by a complete explanation. Possibly, in essentials, Hunter has done all that can be done.

Even if it should happen that all historians become convinced that historical events cannot always (or, for that matter, ever) be shown to be predictable in principle, there is not the slightest reason to suppose that this discipline would disappear. We cannot imagine faculties of history ceasing to attract students on this ground. In fact, if we are to accept the judgment of the historian, J. H. Plumb, most historians do believe this:

So the modern historian is crucified
by this dilemma: he must act like a
scientist although historical objectivity

cannot exist. His work can have no validity except for himself, and, perhaps, for the fellow historians playing the same game by the same rules or perhaps for those men of his age who think and feel like himself. For them his work may deepen their experience as a novel might, or a poem, or even a play like Mr. Albee's.⁸ [Plumb refers to the play, Who's Afraid of Virginia Woolf?]

But students continue to read history under Plumb's "crucified" modern historian; some of them make history their life's work. Why do they do this? Not, surely, because none of them have the talent to do mathematics or become engineers. I suspect that Plumb has given one very good answer to this question although, in the context of his essay, he makes little enough of it. The best of these students of history wish to deepen their own, and their reader's, experience. This is hardly a trivial object of historical research.

Nor can anyone deny that this "deepening" can occur independently of the procedures of hypothetico-deductive

⁸ J. H. Plumb, "The Historian's Dilemma," first appeared in The Sunday Times, March, 1963. Reprinted with additions in J. H. Plumb, ed., Crisis in the Humanities (Penguin Books, Harmondsworth, 1964), p. 30.

science. We need only reconsider Hunter's explanation of the renaissance of painting in New York. Hunter brings together a few facts and by putting these together in a coherent way takes some of the mystery out of what happened in New York. A more detailed study would constitute a progressive deepening of the reader's experience.

Most people go into history because they are satisfied to give "followable" accounts of the past which deepen our understanding of the past. Some, such as Arnold Toynbee, wish to do far more than that; these "metahistorians" do seek out laws of cultural development which will apply to all times and all places (however, even Toynbee qualifies these proposed laws of cultural development to make them, somehow, compatible with a doctrine of "free will"), but the pursuits of the metahistorians are more likely to amaze the historian than to inspire him to do likewise.

The typical historian does not connect "failure to explain" with "failure to show that an event was predictable in principle." It is possible that he should do this but if historians are to be made to make this connection they must, first, give up their usual frame of reference.

The historian sees his failures as like those of a novelist who published a "patchwork quilt," rich in characters and incidents but lacking structure; lacking any evidence that the novelist had any principle of selection or means of tying together his story. "Failure to explain" is equated with lack of discernment in selecting material and "letting the facts speak for themselves" in such a way that they do not speak at all.

For example, the biographer of Arthur Rimbaud who gave us a list of things Rimbaud ate for breakfast in the years 1858-1862 would be wasting his time and ours. This list, resting on no critical acumen, would not speak for itself. But the biographer who explained to us why Rimbaud stopped taking any interest in poetry after 1875 would be telling us something we want to know, if we are interested in the literary history of France.

This example is worth developing for it will bring out the point that when we say that something has been explained we do not necessarily mean that a statement of it can be deduced from some principles. We know, as a matter of fact, that Rimbaud wrote no poetry after 1875; it is quite possible that his last poetry dates from 1873, when

he completed A Season in Hell. We also know that in later life Rimbaud, if the topic of his literary work came up, described that work as "Absurd! Ridiculous! Disgusting!" --this at a time when his literary reputation was growing and some people insisted upon informing him of that fact.

Many people become ashamed of the poetry they have written in adolescence; there is nothing puzzling in that. But it is unusual for an author to condemn work which has been favorably compared with the assured successes of Baudelaire or, as Rimbaud was informed, has led to the formation of a school.

It is also true that some people stop writing even good poetry at a fairly early age. They have no more to say, or they are "burned out," or they become interested in something else. It may well be that Rimbaud had no more to say and there is no doubt that he became interested in other things. But why would this lead him to judge that what he had said was "ridiculous"? Why, in particular, would he refuse to correspond concerning matters of literature?

There is no generally accepted explanation of this behavior but, recently, Oliver Bernard has offered an

explanation which has the merit of fitting the known facts of Rimbaud's life and making sense of his renunciation of poetry and his bitter judgment of his efforts:

Rimbaud, who had shown as a boy a religious nature, being possessed at the age of twelve of "a burning faith, a passionate piety," began to reject both God and the Church just as he was becoming a poet. . . . magic and alchemy were a substitute for religion. Better than that: they were the means of becoming god-like, and therefore "better than" religion. That Rimbaud had this idea, and eventually renounced it, can be deduced partly from "Délires II" in Une Saison en enfer, and perhaps most convincingly of all from Rimbaud's renunciation of literature itself. For if you begin, as a poet, by trying to become as a god, what will you do when your efforts fail: I think that Rimbaud realized that the "magique étude" had failed; and I think his reaction was to reject everything he had done while pursuing it. His pride was monstrous. If all he had wanted to do had been to write poems--a made enough ambition in itself--he would have had no reason either to stop trying to write them or to despise his whole work in later life.⁹

The account is certainly intelligible but it is not easy to see that it is, or should be, transformed into a "scientific" account. We do not discover any precise

⁹Oliver Bernard, trans., Rimbaud (Penguin Books, Harmondsworth, Middlesex, 1962), pp. xxiv-v.

entailment relations. For example, Rimbaud's pride seems to do double-duty. On the one hand, this pride receives a rebuff through Rimbaud's failure to become a god.

(Bernard could have supported his claim that Rimbaud wished to become a god by reference to Rimbaud's correspondence with Izambard and Demeny.) Consequently, we might say, Rimbaud rejects the thing which has hurt him most--poetry. On the other hand, Rimbaud's pride is such that it is a permanent strand in his personality.

Rimbaud's pride is both a feeling and a disposition. Put another way, the term "pride" is not being used as a "precized" term taken over from psychological theory. The more we work on this example from Bernard, the less scientifically satisfactory we find it. Perhaps we can do something about explaining Rimbaud's behavior simply using Bernard's account as a point of departure; a clue to a scientific explanation but no more than a clue.

This is the approach, as we shall consider in detail in the next chapter, that Hempel takes in his analysis of history; an "explanation" like that of Bernard's really does approximate scientific explanation patterns but it is so "loose," there are so many complicating factors,

that we may fail to see the resemblance or the approximation. But if we take Gallie's analysis as basic, we find ourselves describing Bernard's work in this way: Bernard feels what is essential for deepening the reader's understanding of Rimbaud but his "proof" for what he says--aside from the biographical facts he gathers--resides in the extent to which his account squares with these facts and makes the sequence of events intelligible to the reader. He has no crucial experiments to perform and no definitive deductions to make.

But it is not clear that Gallie's analysis of (explanatory) historical narrative is actually an alternative to a "positivistic" analysis.

As mentioned, we might say that Bernard gives us a clue toward a scientific explanation of Rimbaud's bitter rejection of poetry. Or, perhaps, his explanation is a scientific explanation in a "primitive" or "approximative" state. Gallie is certainly right in pointing out that historical narratives may be coherent without being in the form of scientific explanations. But maybe they can be put into this form. Possibly they are in this form although the language of history somehow hides this pattern.

If it were to turn out that historians really make exclusive use of hypothetico-deductive, or probabilistic, patterns of explanation their accounts would, of course, be intelligible or "followable." Perhaps we should say that when an historian fails to give anything less than a scientific explanation he has simply failed to give an explanation or--expressed in a positive way--that he has given us a clue to an explanation or, to a greater or a lesser degree, is approximating the ideal of rigorous, scientific explanation.

In any case, we can at least imagine a narrative structure being put together on the basis of laws. For instance, narratives in natural history are of this kind. One can produce, in straightforward English, a narrative of the eruptions of Mt. Vesuvius which would rest on various physical and chemical laws.

The account would be a "followable story" as well as a scientific exposition in geology. It would have all the essential features of the "logic of historical narrative" except, of course, the feature that expository historical narratives are, in the main, about the doings of human beings. But the important thing is that the history

of the eruptions of Mt. Vesuvius (including an explanation of these eruptions) would be in the narrative form and the explanation pattern abstracted from the narrative would be of the hypothetico-deductive type. The eruptions of Mt. Vesuvius would be fitted into a pattern of events explained in accord with laws. Although it would certainly be arduous in the case of historical narrative to do this, we cannot pronounce the effort impossible on the basis of the features of historical narrative, story-telling or otherwise. The feasibility of this effort can only be assessed by taking up patterns of explanation, or alleged patterns of explanation, and determining, if possible, which of these are most faithful to historical procedure or to the requirements of history as a research discipline.

CHAPTER VI

THE COVERING LAW MODEL OF HISTORICAL EXPLANATION

In 1942, Hempel published what has proven to be one of the most influential articles in the literature of critical philosophy of history.¹ In this article he proposed that an explanation for the occurrence of an event "consists, . . . 'in indicating the causes or determining factors of" the event. "Now the assertion that a set of events . . . have caused the event to be explained amounts to the statement that, according to certain general laws, a set of events of the kinds mentioned is regularly

¹C. G. Hempel, "The Function of General Laws in Historical Explanation," Journal of Philosophy, Vol. 39, 1942. Reprinted in Patrick Gardiner, ed., Theories of History (The Free Press, Glencoe, Ill., 1959), pp. 344-56.

accompanied by an event of Kind E."² More precisely, the scientific explanation of an event consists in:

(1) a set of statements asserting the occurrence of certain events C₁, . . . C_n at certain times and places,

(2) a set of universal hypotheses, such that

(a) the statements of both groups are reasonably well confirmed by empirical evidence,

(b) from the two groups of statements the sentence asserting the occurrence of event E can be logically deduced.³

The model described above is often called the "covering law" model of explanation because it describes an explanation pattern in which whatever is to be explained is brought under or "covered by" general laws; as William Dray, who is responsible for this terminology, puts it:

. . . what the theory maintains is that explanation is achieved, and only achieved, by subsuming what is to be explained under a general law. Such an account of the basic structure of all explanation is sometimes referred to as "the regularity analysis"; but because it makes use of the notion of bringing a case under a law, i.e. "covering" it with a law, I shall often speak of it hereafter as "the

²Gardiner, op. cit., p. 345.

³Ibid.

covering law model."⁴

It is a deductive model of explanation because it requires that the sentence asserting the occurrence of the event to be explained be logically deducible from the preceding sentences, or statements, which assert (a) the occurrence of certain events at certain times and places and (b) universal hypotheses.

The deducibility requirement is "weak" in the sense that a satisfactory covering law explanation need not be in the form of a completed deductive argument in which the conclusion has actually been shown to follow as a logical consequence of the premises. The requirement is met if it is possible to construct a valid proof demonstrating that the conclusion follows as a logical consequence of the

⁴William Dray, Laws and Explanation in History, (Oxford University Press, American House, London, 1957), p. 1. Professor Dray has pointed out that some people may be misled by this terminology; they may suppose that this terminology implies either: (a) covering law explanations make use of only one covering law or (b) the covering law requirement will not be distinguished from the deducibility requirement (William Dray, "The Historical Explanation of Actions Reconsidered," in Sidney Hook, ed., Philosophy and History [New York University Press, New York, 1963], p. 106 and pp. 133-4, note 3).

premises.⁵

According to Hempel all scientific explanations are of the covering law form. He does admit that some scientific explanations are "probabilistic" in character; this is alleged to be so in those cases where the event to be explained is brought under probabilistic laws and its expectation is shown to be of a certain order of probability. In 1942 he did not draw out the implications of his admission. The admission implies that (a) the deducibility requirement need not be met even in its weak form and (b) the covering laws need not hold without exception.⁶ I shall discuss the importance of these implications in a later portion of this chapter.

Hempel maintains that all genuine historical explanations are of the covering law form. History is an empirical discipline; it exhibits precisely the same explanation pattern as do the empirical sciences. History is a science, although it is admittedly in a primitive

⁵Alan Donagan, "Historical Explanation: The Popper-Hempel Theory Reconsidered," History and Theory, Vol. 4, No. 1, 1964, p. 5.

⁶Ibid., pp. 5-6.

stage of its development:

Historical explanation, too, aims at showing that the event in question was not "a matter of chance," but was to be expected in view of certain antecedent or simultaneous conditions. The expectation referred to is not prophecy or divination, but rational scientific anticipation which rests on the assumption of general laws.⁷

As Dray remarks in a discussion of the above passage in his most recent book: what this claim asserts is "a logical or conceptual connection between having knowledge of laws and being able to give explanations that are defensible. And it is argued that what is true of explanation generally must be true also of historical explanation."⁸

Now, whatever the merits of this claim, it is an obvious fact--verified by inspecting any work of history--that historians do not put their explanations into the schema Hempel provides. If, in Ernest Nagel's phrase, the historian "must be armed with a wide assortment of

⁷Hempel, op. cit., pp. 348-9.

⁸William Dray, Philosophy of History (Prentice Hall, Inc., Englewood Cliffs, N.J., 1964), p. 6.

general laws,"⁹ it is notorious that the historian never submits to an inventory of these general laws and that some historians are so disingenuous as to claim that they have been unarmed all the time.

Not only do they not do so, but it is not easy to see how one could construct satisfactory explanations, in a Hempelian form, from their works. Hempel does not deny this fact but he proposes to show that the actual narrative structure of history is compatible with his account of the role of explanations in history and the scientific form into which he insists any genuine historical explanation can be put:

Most explanations offered in history or sociology, however, fail to include an explicit statement of the general regularities they presuppose; and there seem to be at least two reasons which account for this:

First, the universal hypotheses in question frequently relate to individual or social psychology, which somehow is supposed to be familiar to everybody

⁹Ernest Nagel, "Some Issues in the Logic of Historical Analysis," The Scientific Monthly, Vol. 74, 1952; the reference is to the reprint in Hans Meyerhoff, ed., The Philosophy of History in Our Time (Doubleday Anchor Books, Garden City, N. Y., 1959, p. 205.

through his everyday experience; thus, they are tacitly taken for granted. . . .

Second, it would often be very difficult to formulate the underlying assumptions explicitly with sufficient precision and at the same time in such a way that they are in agreement with all the relevant empirical evidence available.¹⁰

Hempel agrees with those who assert that historians generally do not give scientific explanations. He admits, in effect, that the story the historian tells does not take a form which makes it easy to abstract out the explanations and put them into a covering law form. But he does not draw the inference that history is not, at least in tendency, an empirical science or that it is not similar to all the other scientific disciplines in terms of its methodology.

There is a difference between what the historian is trying to do and what he actually achieves in the course of putting forward an explanation: "A resuscitation of the assumptions buried under the gravestones 'hence,' 'therefore,' 'because,' and the like will often reveal that the explanation offered is poorly founded or downright

¹⁰Hempel, op. cit., p. 349.

unacceptable."¹¹

The historian often fails to offer a satisfactory explanation of an event but his failures, however numerous, do not justify the conclusion that history is not a science; his failures, as Hempel sees it, only highlight the fact that the historian deals with matters of extraordinary complexity.

The historian may only be able to give reasons for assuming that the event to be explained was a probable occurrence; it may be impractical for him to list all of the relevant features of a completely satisfactory explanation of the event in which he is interested. Perhaps he simply does not know enough to do so.

These considerations lead Hempel to introduce the notion of an "explanation sketch."

Such a sketch consists of a more or less vague indication of the laws and initial conditions considered as relevant, and it needs "filling out" in order to turn into a full-fledged explanation. This filling-out requires further empirical research, for which the sketch suggests

¹¹Hempel, op. cit., p. 351.

the direction. (Explanation sketches are common also outside of history; many explanations in psychoanalysis, for instance, illustrate this point). . . . A scientifically acceptable explanation sketch needs to be filled out by more specific statements; but it points into the direction where these statements are to be found; and concrete research may tend to confirm or to infirm those indications; i.e., it may show that the kind of initial conditions suggested are actually relevant; or it may reveal that factors of a quite different nature have to be taken into account in order to arrive at a satisfactory explanation. - The filling-out process required by an explanation sketch will, in general, assume the form of a gradually increasing precision of the formulations involved; but at any stage of this process, those formulations will have some empirical import: it will be possible to indicate, at least roughly, what kind of evidence would be relevant in testing them, and what findings would tend to confirm them. In the case of non-empirical explanations or explanation sketches . . . the use of empirically meaningless terms makes it impossible even roughly to indicate the type of investigation that would have a bearing upon these formulations, and that might lead to evidence either confirming or infirming the suggested explanation.¹²

The historian should aim at giving explanation sketches where, as is often the case in history, it is

¹²Ibid., p. 351.

impossible to give complete explanations. From what the historian says in accounting for some past event it should be possible to see what would count for or against his argument; what assumptions he is making use of and how he applies them to the particular case. In the ideal case, his "explanation sketch" will be replaceable by a full-fledged explanation resulting from the filling-in of the former. The former is in the nature of a promise that the latter can be acquired.

Hempel gives one example of what he takes to be the occurrence of an explanation sketch in historical narrative:

Consider, for example, the statement that the Dust Bowl farmers migrate to California "because" continual drought and sandstorms render their existence increasingly precarious, and because California seems to them to offer so much better living conditions. This explanation rests upon some such universal hypothesis as that populations will tend to migrate to regions which offer better living conditions. But it would obviously be difficult accurately to state this hypothesis in the form of a general law which is reasonably well confirmed by all the relevant evidence available. Similarly, if a particular revolution is explained by reference to the growing discontent, on the part of a large part of the population, with certain prevailing conditions, it is

clear that a general regularity is assumed in this explanation, but we are hardly in a position to state just what extent and what specific form the discontent has to assume, and what the environmental conditions have to be, to bring about a revolution. Analogous remarks apply to all historical explanations in terms of class struggle, economic or geographic conditions, vested interests of certain groups, tendency to conspicuous consumption, etc.: All of them rest on the assumption of universal hypotheses which connect certain characteristics of individual or group life with others; but in many cases, the content of the hypotheses which are tacitly assumed in a given explanation can be reconstructed only quite approximately.¹³

In Hempel's example the universal hypothesis is "Populations will tend to migrate to regions which offer better living conditions." The word "tends" reveals that the explanandum ("The Dust Bowl farmers migrate to California") is not deducible from the explanans ("The Dust Bowl farmers suffer from drought and sandstorms which render their existence increasingly precarious" and "Populations will tend to migrate to regions which offer better living conditions").

The explanation sketch is not a sketch of the type

¹³ Ibid., pp. 349-50.

of explanation Hempel originally put forward. The "universal hypothesis" is not strictly universal. Its very formulation shows that it admits of exceptions and, therefore, that we cannot deduce the explanandum from the explanans. In later writings, Hempel gave an explicit description of an alternative covering law model which is compatible with explanation sketches of the type represented in his 1942 paper.¹⁴

The alternative model is a "probabilistic" covering law model. The explanans confers high inductive probability on the explanandum. The covering law requirement is weakened; it is sufficient if the covering law provides what Hempel calls "inductive support" for the explanandum. At the same time the weakening of the covering law requirement entails the abandonment, for this model, of the deducibility requirement. Because the law states only long run relative frequency distributions, it cannot serve as a

¹⁴My comments are based on C. G. Hempel's recent article, "Reasons and Covering Laws in Historical Explanation," in Sidney Hook, ed., Philosophy and History (New York University Press, New York, 1963), pp. 143-163. See page 145 of this article for the explicit formulation of this alternative covering law model.

premise in a deductive argument (ignoring the trivial case where a statement of the law is used as an unnecessary premise in a deductive argument which is independently valid).

The universal hypothesis in Hempel's explanation sketch is a co-variation. The statement of it is a statement to the effect that the majority of populations centered in areas which offer unsatisfactory living conditions migrate to regions which offer better living conditions. This statement is quite possibly false but for the sake of the discussion I will assume that it is true.

If it is true, does the fact that it is true explain the behavior of the Dust Bowl farmers? Alan Donagan remarks that:

If your task is to explain why a given event E occurred, rather than did not occur, then your explanans must exclude the possibility that E did not occur; but, if your explanation is not deductive, i.e., if its explanans does not logically entail its explanandum, then it will not exclude that possibility, and so will not explain why E did occur.¹⁵

If Donagan is correct, the only satisfactory

¹⁵Donagan, op. cit., p. 7.

Hempel's explanation sketches would be those which could satisfy the requirements of Hempel's earlier covering law model of explanation. If the sketches rest upon co-variation hypotheses to begin with, it would be logically impossible, by filling in these sketches, to transform them into satisfactory scientific explanations.

Hempel does not admit that, in an explanation, the explanans must exclude the possibility that the event to be explained does not occur. He argues that an explanation may consist in showing the likelihood of an event.¹⁶ He takes the example of a joint rolling of four dice. The probability that four standard dice will yield a total of more than four dots on a joint roll is .9992. From this he concludes that (a) on a joint roll of four dice we cannot preclude the possibility that the dice will yield a total of four dots and (b) we can explain the fact that they do not--if they do not--because the probability that they will not is close to 1.¹⁷

¹⁶Hempel, "Reasons and Covering Laws . . .," pp. 144-145.

¹⁷Ibid., pp. 144-145. "(b)" is implied by what Hempel says on page 144. "(a)" is implied by what Hempel says on page 145.

We are left with the peculiar implication that if four dots come up (the probability of this happening being .0008), there is no explanation for this event. Hempel does not draw this implication but it is an unavoidable one. The only way to avoid the implication is to deny "(b)"; and there are perfectly good reasons for denying "(b)." A probabilistic explanation does not deal with an event; it deals with a range of events.

A probabilistic explanation does not tell us why four dots came up on a given joint roll any more than it tells us why they did not. If we are interested in a specific result, we must seek the explanation of that result elsewhere. Alan Donagan--to whom I am indebted for the general line of criticism developed here--gives the impression that he thinks that we cannot explain a specific roll at all; neither probabilistically nor any other way. As he puts it: "You cannot explain luck."¹⁸

He is, I believe, confused on this point. Dice, since they are macrophysical objects, presumably obey all the laws of physics which are relevant to their behavior.

¹⁸Donagan, op. cit., p. 8.

In theory--if we knew all there was to know about the conditions under which a die is thrown--we could predict what die-face would come up. The same holds for coin tosses and roulette spins (the context of Donagan's remark about luck). In theory (although admittedly not in practice) it is possible to explain why a coin came up heads or number 00 came up in roulette. But this does not weaken the general line of criticism; the explanation of a specific event--if it can be had--will not be a probabilistic explanation.

Hempel's modified model of explanation serves quite a different purpose. The general hypothesis, or hypotheses, show us that an event to which the general hypothesis is related is typical or atypical. To return to Hempel's example quoted earlier, the fact that the Dust Bowl farmers migrated to California is not an unusual fact. If someone asked why they migrated, he would usually be satisfied with the explanation that their land was poor and that they sought out better land. Since we all know that some people stay on poor land--they may be prevented from migrating, or they may not know that there is better land to be had--no one would expect there to be a one-to-one correlation

between people having poor land and abandoning it. But there is, we will assume, a fairly high correlation or co-variation. It is legitimate to say that the Dust Bowl farmers' migration was something only to be expected. In those cases where we know that the likelihood of an event is very high, we may say that the event was only to be expected. An explanation in the form of Hempel's modified model may show that an expectation was reasonable.¹⁹

It may show why people would expect something to happen, but it will not explain the happening itself.

Hempel's example of the dice serves to show that a given expectation is reasonable. It explains the expectation by showing that the probability of the occurrence of what is expected is very high. However, there is a difference between this case and the example of the Dust Bowl farmers. In the latter example the general hypothesis indicates that the migration was typical but it does not actually serve to show why it was only to be expected.

In Hempel's dice example, we not only know that the inductive probability of an event is very high (.9998),

¹⁹Ibid., p. 8.

but we also know how the probability is calculated. We know why the probability is what it is. Because of this we also know why the expectation that four dots will not come up is a reasonable expectation (an expectation which can, of course, be upset).

In Hempel's historical example, the "general hypothesis" is not a probability hypothesis. All it tells us is that a majority of people who have been in a certain situation have done such-and-such. Even if the "general hypothesis" stated that a certain high percentage of people in a given situation always went on to do such-and-such, we would not know why this was so.

It is unreasonable to demand that every law be explained by means of other laws and it may seem that all I am saying is that Hempel's "general hypothesis" is not self-explanatory; a fact with which neither Hempel nor anyone else would quarrel. But what I am actually getting at is the fact that the "general hypothesis" is not a law at all:

. . . it should not be surprising that in history we cannot, for example, find laws which "cover" the case of a particular migration of population in the sense that there is a law of population migration

such that this case is an instance of it. Rather, the laws which we could expect to find (if we are to find explanatory laws) would be in one sense more general; in another sense, they would also be more limited than the event which they are to explain. They would be more general, since they would presumably also serve to explain other types of case, and not only population migrations; they would be more restricted because they would not concern all of the aspects of the complex event designated as a population migration, but only some one aspect of it. Thus, for example, we might expect some social psychological law to be useful in explaining a population migration, but such a law, if it were genuinely a law of social psychology, would also be relevant in explaining particular events which are not population migrations. At the same time such a law would be restricted to dealing with some one factor in the population migration, and not with the complex event as a whole.²⁰

If we want to know why the migration of the Dust Bowl farmers was only to be expected, we shall have to find out why the "general hypothesis" is true. This means that we must determine what factors account for the co-variation.²¹ If we are able to articulate the laws which

²⁰Maurice Mandelbaum, "Historical Explanations: The Problem of 'Covering Laws,'" History and Theory, Vol. 1, No. 3, 1961, p. 235.

²¹The argument that correlations must themselves be made intelligible before they can be used as explanatory principles is a point I owe to Raymond Aron,

account for the co-variation we are, at the same time, able to show why the migration of the Dust Bowl farmers was only to be expected. Showing what factors account for the co-variation is equivalent to showing why a probability is of a certain order. It is true that this particular co-variation, "Populations will tend to migrate to regions which offer better living conditions," seems to rest on common-sense. The "factors" do not appear to be very mysterious. People must eat to live; if people are starving or living on a very poor diet and discover that there is a place to which they are free to go where they will not starve or will have an improved diet, we expect them to go to that place because, after all, hunger-pangs will drive one to take some action. But I am not concerned with how easy it may be to infer the factors behind a correlation but only with the methodological point that a correlation does not explain anything; it must itself be explained.

Hempel's example of an "explanation sketch" fails on two counts. It does not permit us to deduce the

Introduction to the Philosophy of History, translated by George J. Irwin (Beacon Press, Boston, 1962); see the scattered remarks, pp. 205-213.

explanandum and it does not permit us to see why the explanandum was a typical event. It would hardly be fair, or profitable, to torture one example--as I have done--if Hempel had better ones to put forward. The fact is, however, that he does not.

Nonetheless I think it is possible to show that something like Hempel's covering law model is employed in history. The model I shall propose--which I shall attempt to show is exemplified in history--is a weakened form of Hempel's hypothetico-deductive model. The deducibility requirement is retained but the covering law requirement is modified. Before explaining just how the covering law requirement is modified, I shall give what I take to be an historical explanation sketch whose "logic" is that exemplified by the model.

Alan Barker, in his book on the American Civil War, remarks that "Lee, even if he had had the resources, could never have fought as Grant did."²² He means, as the context makes clear, that Lee would not have had the

²²Alan Barker, The Civil War in America (Doubleday Anchor Books, Doubleday & Co., Garden City, N.Y., 1961), p. 123.

stomach to wage a war in which civilians as well as the opposing military forces were fair game: ". . . modern total war faced the traditional conception of war as a contest of skill, finesse and chivalry."²³

Since Lee's attitude weakened the chances of the South in the war with the North, it is of historical importance and it is not surprising to find that Barker offers an explanation:

. . . if they had adopted the policy of ruthless retaliation which Sherman was to pursue in the last year of the war, victory might have come nearer. But most Southern generals were gentlemen brought up to believe that war was chivalrous, with nothing retaliatory, destructive or total about it. The earlier Union generals had confirmed Lee in his belief that the war was to be fought on these terms; he was shocked and appalled when confronted by the brutal military machine directed by Grant and Sherman . . .²⁴

Lee was brought up to believe that war was chivalrous. By the time Lee was confronted with the tactics of Sherman and Grant, it was too late for him to adjust to the new situation. Can we deduce the explanandum, "Lee could

²³Ibid., p. 144.

²⁴Ibid., pp. 144-5

never have fought as Grant did," from Barker's explanation? The obvious answer is no. But would it be possible to deduce the explanandum from what Barker says given the knowledge of why what Barker says is true? The answer is no longer obvious.

Barker's explanation gains its initial plausibility from the way in which he indicates that Lee's behavior was typical. He had the attitudes that one would expect someone brought up as he was to have. However, Barker does not tell us why people have the attitudes they are brought up to have. As an historian, he would have no reason to do so. But the question of how attitudes are "introjected" or "internalized" is not an easy question to answer. If we look for an answer we look to such disciplines as physiology, psychology, sociology, and psycho-analysis. These are the disciplines which provide us with the factors that account for the correlation between having an attitude and being brought up to have it. This constitutes my first point: if we wonder why people have the attitudes they are brought up to have (and are not concerned with what justifications there may be for these attitudes--how the people would defend them, etc.), we turn to the generalizing

sciences if we are dissatisfied with the offerings of commonsense.

If we discover the laws that account for the correlations we see why Lee's situation was typical. But we do not, as yet, see why he was in the situation that he was in. The deducibility requirement is not satisfied. It may appear that it is impossible to satisfy it. We know, as a matter of fact, that some people do not believe what they are brought up to believe.

But if the deducibility requirement cannot be met, it is hard to see how Barker can be so sure about how Lee could have behaved. He does not suggest that Lee had any option in the matter. I think it is necessary to reconstruct Barker's argument to see why he treats Lee's attitude as an unavoidable one. Barker takes it that there was nothing about Lee--no special characteristics--which would have interfered with the internalization of beliefs. The correlation to which Barker points is not one-to-one. It is, simply, an empirical correlation. But the laws which account for the correlation make use of a "ceteris paribus" clause. These laws--assuming there are such, and I certainly assume that there are--apply only "other things

being equal." The empirical correlation does not make use of the "ceteris paribus" clause. Exceptions to the empirical correlation are not necessarily exceptions to the laws which account for the empirical correlation.

Barker's explanation is effective only if he does, rightly, assume that there was nothing special about Lee which would lead to a resistance to the socialization process to which he was exposed. If Barker is correct, the exceptions to the empirical correlation need not matter. Lee's behavior comes under the laws which account for the high correlation. In principle, it would be possible to deduce the explanandum by treating Lee's case as non-deviant.

Professor Dray remarks that: "When we subsume an action under a law, our approach is that of a spectator of the action; we look for a pattern or regularity in it."²⁵ An explanation of this type is what Dray refers to as a "non-rational" explanation.²⁶ I would suggest that Barker

²⁵William Dray, Laws and Explanation in History, p. 140.

²⁶Ibid., p. 138.

is a spectator of Lee's behavior (in this context his disposition to act in a certain way and to be unable to act in another way).

He does not subsume Lee's attitude under a law because the correlation to which he points is not itself a law; it does no more than indicate that Lee's behavior was typical. (And, after all, Lee's behavior is one of the cases which go into the correlation itself.) It is not the task of the historian to find the laws which account for the correlation. In this respect he is not a consumer of laws. But I think that Barker's explanation would satisfy us only if we thought that the correlation could be made intelligible. To make it intelligible is to discover the factors that account for it. I think that Barker believes that if these factors--certain laws of learning theory, etc.--were scientifically formulated we could see that they would apply to Lee's case. Lee represents a personality-type which internalizes norms, "other things being equal." This is what I mean by saying that if Lee's case is treated as non-deviant, it is possible to deduce the explanandum.

Barker assumes that nothing interfered with Lee having this attitude. If he is going to stand by his

remark that Lee could never have fought as Grant did, I think he is committed to the notion that Lee's attitude can be explained within a hypothetico-deductive framework. He would not attempt this, but the force of his explanation depends upon the notion, explicitly or implicitly accepted, that such a thing could be done. If we do not believe that various non-rational factors account for Lee's attitude--if we think that he was a special case or had special reasons for what he did--we will look for another type of explanation. I do not suggest that Barker's explanation is a sound one, but only that it presupposes the possibility of giving a Hempelian explanation.

Barker's explanation is satisfactory if, and only if, the factors which account for the correlation Barker calls attention to are sufficient, in conjunction with the knowledge that Lee was of a certain psychological type, to permit the inference that Lee would have a certain attitude. The explanation could, it should be mentioned, easily degenerate into a tautology. If we held that Lee's attitude has to be in accord with the laws simply because it is typical, we fall into this trap.

We must make it possible to falsify the laws. It

is this that Professor Mandelbaum called our attention to in the quotation I have already given. If the laws apply to Lee's case they must also apply to the cases of other people who are in Lee's position. We do not demonstrate this by taking out of the empirical correlation the subset of those cases in which a certain kind of internalization process and a certain kind of attitude are found together. We must see that if we place other people in Lee's position they will behave as he behaved. The "ceteris paribus" clause must be restricted. This means that we must state some satisfiable conditions whose satisfaction commits us to the proposition that the man finding himself in those conditions will have the attitude that Lee had. These conditions may be, in part, the "blueprint" for a psychological type which Lee may be alleged to exemplify but if the conditions are met and a man who is of Lee's psychological types, as defined by these conditions, fails to have Lee's attitude we must be prepared to give up the explanation. If we keep weakening the explanatory model to cover exceptions, we finally empty it of all content or empirical significance.

There is one final point to be mentioned on this

score. When I talk of Lee's attitude, I treat the attitude as a shareable one. Similarly, I assume that Barker is concerned to explain Lee's attitude toward war; not toward a specific set of wars. This discussion is not meant to have any bearing on the problem of explaining unshareable attitudes (if there are such) or of explaining attitudes toward specific events which circumscribe the expression of these attitudes.

The next question is: can we test Barker's explanation? In a sense it has been tested. Under the American occupation, the Japanese were brought up to have a horror of war. The system of instruction has been changed because (a) quite a few of the younger Japanese came to have this attitude and (b) the attitude no longer fits in with America's Asian policy. But in another sense it has not. We do not know why it is that some Japanese have found it easy to accept war, despite the fact that they were brought up under the pacifist period, whereas others find it difficult or impossible to "depacifize" themselves. To retreat to an honored sanctuary, "not all the factors are known," as the saying goes.

Yet, insofar as we consider explanations of

Japanese behavior from a non-rational vantage point, I think we are committed to the notion that there is a scientific explanation for the attitude-patterns of the Japanese. In any event, we know how to set up an educational or --as I would prefer to call it--conditioning system, which will favor the emergence of one attitude over another. To some extent we are even capable of setting up reconditioning processes which will offset the results of a conditioning process we have previously set up. This ability to control human behavior (I do not wish to exaggerate the scope of our "control potential") gives us some confidence in the notion that there are laws of human development. But it is quite a step from practical manipulation to the discovery of the principles which account for the possible success of the attempted manipulation.

It is my contention that it is our confidence in the possibility of discovering such laws which makes us accept Barker's explanation if we do accept it. The relationship between Hempel's original model and my modified version of it can now be spelt out in detail. If Lee's behavior is non-rational we explain that behavior by an appeal to laws or we explain that behavior on the assump-

tion that there are laws that account for it. In the case where we make the assumption that there are laws which will account for Lee's attitude, we produce an "explanation sketch." But the sketch has no "universal hypothesis" as premise; where, that is, we understand a "universal hypothesis" to be a law, whether strictly universal or probabilistic. We accept the sketch as an explanation sketch because we believe that the universal hypothesis, or hypotheses, could be supplied.

Obviously, our assumption is justified only if there are such laws. The assumption is reasonable only if we can point to signs which usually indicate the presence of such laws. Co-variations are often, although--as we well know--not always, a sign or indication of the presence of laws. The ability to get similar results under similar conditions is another indication. Finally, if a co-variation holds up for a very long period of time, we are unlikely to think that it is a "fluke."

The historian need not (may not be able to) deduce the explanandum. He must--if he is seeking to explain non-rational behavior--be able to indicate the signs of the presence of laws. If we think he is successful in this, we

will think that it is theoretically possible to deduce the explanandum from a completed explanation in which reference was made to universal laws. In any event, it would not be his task to provide the deduction but he would be committed to the proposition that such a deduction would be possible if we knew enough.

It must be admitted that this is a very weak-sounding conclusion. Even if it is true that historians sometimes presuppose that an event, or attitude, or personality characteristic which is of historical interest can be inferred by means of laws, it does not follow that they are correct in making such a presupposition.

There is no case in which such a deduction has ever been carried out and shown to hold for all other cases of the same type. There is no "existence proof" for even a single Hempelian historical explanation. Why should we think that historians are correct in supposing that there are any historically useful Hempelian explanations?

The only answer I can give is that we presuppose that non-rational behavior can be accounted for by the discovery of laws. To take a simple case, we account for reflex action by means of laws. An agent is, let us say,

made to blink his eye. We can explain why he blinked his eye by listing the conditions under which he blinked his eye and stating the laws which, in conjunction with these conditions, would permit us to infer that he would blink his eye. This is a very trivial case, but it brings out one point. The event can be straightforwardly explained because the environment in which it takes place--say, an optometrist's office--is causally "neutral." The conditions of the epoch in history in which the blinking takes place can safely be ignored.

We are, to some extent, successful in bringing non-rational behavior (reflex actions, certain psychotic symptoms, etc.) under covering laws. We suppose that non-rational behavior in history can also be treated as coming under laws. Admittedly, the conditions in which such behavior was manifested are gone; our information about these conditions is always incomplete. But there are regularities in human life, some of which enter into history, and it seems reasonable to suppose that some of the instances of these regularities could be inferred from laws if we had sufficient information.

My defense of Hempel's model must certainly appear

to be a rather unusual one. Instead of arguing that historians (at least sometimes) apply or consume laws, I argue that, sometimes, they assume them and that the assumption is often no stronger than the notion that there must be laws covering the case, although no one knows what those laws are.

Michael Scriven has proposed a modification of Hempel's model in which the deducibility requirement is satisfied but in which the covering law requirement is replaced by a "normic" requirement. His modification has the advantage over mine in that the "normic" requirement is satisfied by a known "normic" hypothesis. In my model the covering law requirement is presumed to be satisfied by laws which we may be in no position to formulate. His model is similar to mine in that he holds that it is no part of the historian's task to establish a "normic" requirement and that it is not necessary for the historian, in giving an explanation, to mention the "normic" hypotheses the explanation presupposes (or is justified by). But this does not undercut the advantage of Scriven's model over mine. The historian could give the "normic" hypotheses if he were called upon to justify an explanation

which Scriven's model legitimates. On my view the historian may be unable to state the universal hypotheses which back up his explanation-sketch.²⁷

Scriven gives as an example of a normic statement the proposition, "Power corrupts." He holds that propositions of this type have been misunderstood. "Power corrupts" is not a universal statement expressing a universal hypothesis. Some people placed in positions of power do not become corrupted. There are counterinstances to the hypothesis that power corrupts.

This might make it look as if "Power corrupts" were a co-variation statement in the form: "There is a high correlation between people being placed in positions of trust and becoming corrupt." If this be so, the co-variation statement would only, at best, be a sign that certain laws were operative; it would not be one of these laws and the explanation for an individual case of corruption in a situation of power would not rest upon the correlation. The correlation would be a statistical statement and these,

²⁷Michael Scriven, "Truisms as the Grounds for Historical Explanations," in Gardiner, op. cit., pp. 443-75.

as Scriven puts it, ". . . are too weak--they abandon the hold on the individual case."²⁸

A "normic" statement is neither universal nor statistical. What then is it? According to Scriven, it is a statement of what happens unless certain exceptional circumstances obtain.²⁹ I explain, let us say, Stalin's corruption--manifested in the Moscow trials, the principle that the relatives of a traitor may be punished along with the traitor, etc.--by the power that he wielded. (I am not, we will say, trying to explain the specific manifestations of that corruption. I would not know why, just before his death, Stalin was planning a pogrom simply because I knew that he had become corrupt.)

If someone asks me why Stalin's power led to Stalin becoming corrupted, I make the normic assertion that "Power corrupts." This backs up my explanation, although, on Scriven's view, it forms no proper part of it. If the normic assertion is itself challenged on the grounds that some people have held power without being corrupted, I

²⁸ Ibid., p. 467.

²⁹ Ibid., p. 469.

will answer that there were certain exceptional circumstances.

Scriven, because of the argumentational procedure sketched in above, calls normic hypotheses "truisms." But they are non-analytic truisms. If the cases in which power corrupted came to be very rare, we would give up the normic assertion, "Power corrupts."³⁰ To take another example of a normic assertion, "Other things being equal a greater number of troops is an advantage in battle" is a proposition which we can imagine giving up. The techniques of fighting a war may so change that this normic assertion will be abandoned.

Scriven argues that these normic assertions have the same function in history that laws have in the natural sciences. But he denies that they are laws. Some of them, it is true, "seem to state evaluative norms of behavior rather than empirical regularities,"³¹ and these can be distinguished from the laws of natural science.

But Scriven states that some normic statements may

³⁰Ibid., pp. 466-7.

³¹Dray, Philosophy of History, p. 16.

be empirical.³² It is this class of normic statements which approximate the function of universal hypothetical statements in Hempel's model. My question is: what distinguishes an empirical normic hypothesis from a universal hypothesis? Scriven appears to suggest that the distinction rests upon the degree to which an empirical normic hypothesis is immune to falsification. But they are not completely immune.

On the other hand, a scientific hypothesis is not necessarily less immune to refutation than an empirical normic hypothesis. There is a place for the "ceteris paribus" clause in both scientific hypotheses and in empirical normic hypotheses. To take an example, if, in a game of pocket billiards, one strikes the cue-ball just below its axial center, it is supposed to stop at the point of collision with another ball. This behavior can be explained by means of the principles of Newtonian kinematics. But quite often the cue-ball does not "stop dead" as it is supposed to do. We are very unlikely to give up

³²Michael Scriven, "New Issues in the Logic of Explanation," in Hook, op. cit., p. 345.

the principles of Newtonian kinematics because of this. We suppose that these hold "other things being equal." We look for evidence that other things were not equal. If the worst comes to the worst, we suppose that the player did not actually hit the cue-ball at the right spot, even if, to all appearances, it looked as if he did.

There must, of course, be circumstances under which we would be prepared to give up Newtonian kinematics and the very possibility of revolutions in physical theory show that there are such circumstances; that the "ceteris paribus" clause is not stretched to the point that the assertions of Newtonian physics are utterly trivial. But this is also the case with an empirical normic hypothesis. We can imagine giving it up. The circumstances under which we expect it to have application are not as clearly defined as the circumstances under which we expect Newtonian kinematics to apply. But this vagueness does not justify, in my opinion, any sharp distinction between a normic empirical statement and an empirical law.

Scriven would interpret Barker's explanation of Lee's behavior in this way. Lee could not fight Grant's kind of war because he was brought up to believe that wars

were chivalrous affairs. We now have the historical explanation proper. The justification for the historical explanation resides in the empirical normic assertion, "People have the attitudes they are brought up to have--other things being equal." Here we stop. In Lee's case, other things were equal. The deducibility requirement is satisfied because Lee's behavior is "covered by" the normic empirical hypothesis.

But the empirical normic hypothesis could be falsified. An unrestricted use of the "ceteris paribus" clause would prevent this from happening--every conceivable counterinstance could be treated as an instance of a case in which other things were not equal--but at the cost of converting the hypothesis into an analytic statement. Therefore, however we manage it, we do restrict the use of the "ceteris paribus" clause. In some way we allow for the possibility that the normic hypothesis will have to be given up. But how can we allow for this if we do not think that certain conditions make for people having the attitudes that they are brought up to have and certain other conditions make for the opposite? The historian's inquiry may stop with the citation of a normic "truism." But

"truism" is hardly the word to describe a falsifiable assertion--one which, moreover, invites further inquiry. It is not, after all, obvious that people have the attitudes that they are brought up to have, "other things being equal." I would say that the "truism" is either itself a law, however vaguely formulated and however hard to confute, or suggests laws which account for our relative "safety" in supposing it to be true.

I do not wish to exaggerate the differences between Scriven's model, to which my own owes much, and the reformulation of Hempel's model which I have proposed. But there is this key difference. I think that Scriven's "truisms" (where these are empirical) always call for further explanation. I do not think that it is any part of the historian's task to provide further explanations but I think that it is somebody's job to do so. I think our confidence in an historical explanation--when the explanation rests upon regularities--reflects our confidence in the notion that it is theoretically possible to explain these regularities.

CHAPTER VII

THE MODEL OF RATIONAL EXPLANATION IN HISTORY

Some analysts have been unconvinced by the claims put forward for the covering law model not only because the model seems so remote from much, if not all, historical practice but also because they think the "logic" of scientific explanation does not exhaust the "logic" of explanation. A number of efforts have been made to abstract from history, or from a comparison of historical procedure with everyday manners of giving explanations, another model of historical inquiry and permit us to distinguish, at least partially, the methodology of history from that of the sciences, pure or applied.

R. G. Collingwood developed an alternative model of historical explanation to demonstrate that historical method is quite different from the method of the generalizing sciences and his model--which has been subject to more than one interpretation--has been an influence on more

recent work in the philosophy of history. Judging from his An Autobiography, he put forward his alternative model of explanation on the basis of his experience as a practicing historian.¹

In writing his sections of "Roman Britain," the first volume in the Oxford series on the history of England, Collingwood found himself confronted with situations in which it was necessary to "recreate" the thought of an agent in order to understand, or "explain," the events brought about by the activities of that agent. He gives the following example:

Julius Caesar, we are told, invaded Britain in two successive years. What did he do it for? The question is hardly ever asked by historians; and I can remember none who has tried to answer it scientifically, that is, by means of evidence. There is, of course, no evidence to speak of except that contained in Julius Caesar's own narrative. There he never says what he meant to effect by his invasions of Britain. It is the fact of his silence that constitutes our chief evidence as to what his intention was. Whatever he meant to bring about, his intention was one which he decided to conceal from his readers. In the light of a general acquaintance with the Commentaries, the likeliest explanation for this concealment was that

¹R. G. Collingwood, An Autobiography (Oxford University Press, Amen House, London, 1939), pp. 120-46.

whatever his purpose had been he had failed to achieve it. I then compared the strength of his expeditionary force with that of the army sent over by Claudius, nearly a century later, and this settled it. Caesar must have intended no mere punitive invasion or demonstration of force, like that of his German expedition in 55, but the complete conquest of the country. Once more, this view of mine may be mistaken; but future historians will have to reckon with the question I have raised, and either accept my answer or produce a better one.²

Collingwood's explanation is the result of an attempted "re-enactment in the historian's mind of the thought whose history he is studying."³ He places himself in Caesar's position: suppose that Caesar had failed in two expeditions; would he be likely to call attention to the fact in his Commentaries if there were no pressing need to do so? Collingwood concludes that Caesar would not be likely to advertise his failures (unless they were to be crowned by ultimate success; e.g., the campaigns against Vercingetorix); Caesar's intention--the conquest of Great Britain--is seen to make sense of two otherwise puzzling

²Ibid., p. 131.

³Ibid., p. 112

affairs: (a) the silence of Caesar's Commentaries and (b) the strength of the forces Caesar employed on his excursions into Great Britain.

There may be other explanations of these facts but no very good ones, according to Collingwood, have been proposed. His reconstruction of Caesar's thought, or Caesar's intention, is not incapable of improvement or necessarily infallible; Collingwood claimed only to have produced a plausible reconstruction of Caesar's thought; he did not claim to have the reconstruction. A claim of that order would involve the demonstration that only that reconstruction would account for all relevant evidence.

Collingwood would, however, claim that his explanation, whether or not correct, was complete. When we see why the man did what he did (in terms of his reasons, intentions, goals, or, generally speaking, thought) we are finished with our explanation. We need not, perhaps it is absurd to try, bring his thought under covering laws.

I do not propose here to give a general analysis of Collingwood's views which are far from as simple--or as easy to make coherent--as the example from his researches on Great Britain might suggest. I wish only to bring out

the point that Collingwood gave an impetus to non-scientific theories of historical explanation by raising questions about the assumption that all explanations are, if both satisfactory and reasonably complete, of the covering law type. His own alternative to this assumption which he rejected--insofar as one can state it without involving oneself in disputes about what Collingwood really meant by such things as "recreating" thought--came rather close to what one thinks of as at least one type of everyday explanation. What is more common than to explain a man's silence about an affair he has been engaged in than by reference to the hypothesis that the affair hasn't worked out and that the man's pride prevents him from referring to that fact, or to say that the man simply does not wish to appear unsuccessful but does wish to appear successful; that if he had been successful he would have mentioned his gain in the affair; since he is silent he must have been unsuccessful?

Collingwood's model--even in the caricatured form in which I have presented it--suggests lines of approach quite different from those of Hempel and the "positivists" in general. I shall now consider one of these contemporary approaches.

In his book, Laws and Explanation in History, William Dray presented a non-Hempelien model of historical explanation which he called the model of "rational explanation."⁴ It was, and is, Professor Dray's contention that a great many historical explanations are rational explanations rather than disguised, or defective, Hempelian explanations. Because Dray's statement of the properties of a rational explanation is not, in all its aspects, accepted by other philosophers of history who may believe that there are non-Hempelien rational explanations, I shall usually refer to Dray's model as the Dravian model and examples of explanations which fit the model as Dravian explanations.⁵

⁴William Dray, Laws and Explanation in History (Oxford University Press, Amen House, London, 1957), pp. 118-55. Professor Dray, in his book, also considers other types of explanation. I center on rational explanations because these are presented as direct counters to Hempel's model which, like Hempelian explanations, seek to answer why questions.

⁵I follow Alan Donagan in this usage; see "Historical Explanation: The Popper-Hempel Theory Reconsidered," History and Theory, Vol. 4, No. 1, 1964, p. 23.

A Dravian explanation is an explanation of human actions. It consists in showing that what an agent did was the thing to do in the light of the agent's situation, as the agent conceived it, and goals. It shows the "point" or "appropriateness" of an action.

To take an example, when Marx proposed that the General Council of the International be moved from London to New York everyone, as Isaiah Berlin expresses it, "realized that this was tantamount to the dissolution of the International. America was not merely infinitely distant from European affairs, but insignificant in the affairs of the International."⁶ Why did Marx do it? E. H. Carr treats Marx's proposal as a device to save the International from the Bakuninists:

In the United States, at any rate, the Bakunist danger would be sufficiently remote; and the Council, if impotent, would at least be orthodox. . . . The six Bakuninists abstained from voting. In fact, they had nothing to gain or lose by the decision. Marx had killed the

⁶ Isaiah Berlin, Karl Marx, His Life and Environment (A Galaxy Book, Oxford University Press, New York; 2nd ed., corrected, 1961), p. 254.

International. But he had saved it from them.⁷

Isaiah Berlin offers a similar, although more complex, explanation:

Marx gave no explicit reason for this proposal, which was formally moved by Engels, but its purpose must have been clear enough to all those present. He could not operate without the loyal and unquestioned obedience of at least some sections of the body over which he ruled: England had seceded; he had thought of moving the Council to Belgium, but there, too, the anti-Marxist element was becoming formidable; in Germany the government would suppress it; France, Switzerland and Holland were far from reliable; Italy and Spain were definitely Bakuninist strongholds. Sooner than face a bitter struggle which could end at best in a Pyrrhic victory and destroy all hope of a proletarian unity for many generations, Marx decided, after ensuring that it did not fall into Bakuninists' hands, to allow the International to die peacefully.⁸

Both Carr and Berlin give what Professor Dray would describe as "rational explanations." Each points out that Marx had a reason, or reasons, for his proposal. The proposal itself is no longer unintelligible to us because

⁷E. H. Carr, Michael Bakunin (Random House, New York, 1961), p. 448.

⁸Berlin, op. cit., p. 254.

we see it as a means of dealing with a Council which threatened to slip out from under Marx's control. Berlin indicates how alternatives which would both keep the International a vigorous organization and under Marx's control were blocked off. Since Marx believed, rightly or wrongly, that a take-over by the Bakuninist faction would do great harm to the proletarian movement, he had a choice between proposing the dissolution of the Council (a proposal which would probably have been defeated) and proposing a removal which would, I take it, at least preserve the symbolic value of the International.

Marx's proposal had a point. We see its point when we reflect upon the situation in which Marx believed he was placed.

Given Marx's goals, the reasons we suppose Marx to have had (Marx, as Berlin tells us, offered no explicit explanation for his action) can be seen as good reasons for the action he took. If we are correct in attributing these reasons to Marx we are free to say that they explain Marx's proposal. We think they are correct because they fit the available data. Marx and Bakunin not only clashed as personalities but they also clashed as political theorists.

They could not have formed a stable duumvirate. Marx was keenly aware of that fact.

The explanations of Carr and Berlin are not in Hempelian form; there is no reference, however oblique, to covering laws which are somehow supposed to be relevant to Marx's action. (I realize that to some it may seem odd to classify a proposal as an "action" since proposals are something to be acted on; but a proposal does represent initiative and, in this sense, it is a human action.) Nor is there any indication that a weakened Hempelian model would mirror the explanatory patterns of Carr and Berlin. They do not suggest that Marx's action was only to be expected in the light of certain probabilistic laws; nor do they suggest that Marx's behavior is typical in a way that might be taken as a sign of the presence of laws from which, in conjunction with the relevant circumstances, his action could be inferred.

We may nonetheless think that there must be some generalization implicit in the accounts of Carr and Berlin. How can we be sure that their explanations are good ones if these explanations do not, however obliquely, call attention to characteristic features of our experience? John

Passmore remarks that, "At the everyday level, then, any explanation is intelligible provided that it relies upon a familiar sort of connection."⁹ But what is the "familiar sort of connection" upon which the explanations of Carr and Berlin rely? The familiar sort of connection universal hypotheses provide is, presumably, absent. What other kind of connection could there be?

Professor Dray proposes that rational explanations do have a principle of connection which is something like a generalization, although "generalization" would be a misleading term to apply to this principle. He calls this principle a "principle of action." A statement of a principle of action expresses a judgment of the form: "When in a situation of type $C_1 \dots C_n$ the thing to do is x ."¹⁰

⁹John Passmore, "Explanation in Everyday Life, in Science, and in History," History and Theory, Vol. 2, No. 2, 1962, p. 112.

¹⁰William Dray, op. cit., p. 132. The material in quotation marks is cited verbatim from Dray, but the rest of the sentence is an interpretation of lines 19 to 25.

A principle of action is not an empirical law nor a "stand-in" for an empirical law (a sign that one or more laws are operative). It does not tell us what people will, or are likely to, do, or fail to do, but only what they ought to do given their goals and circumstances. In a Dravian explanation a principle of action serves the same function as a general hypothesis in a Hempelian explanation or a normic statement in a Scrivenian explanation. The principle of action gives us confidence in the explanation offered.

To see how this is so, we shall reconsider the explanations of Carr and Berlin. If we accept their explanations on what basis do we do so? Presumably, we accept them because they show us that what Marx did was what we would have expected any reasonable person to do who had goals like those of Marx and was placed in circumstances similar to those of Marx. There is a connection between what Marx did and what we would expect a man similar in interests and circumstances to Marx to do, if he were reasonable. Whatever principle of action is invoked has a judgmental character. We, on the basis of our conception of what it is rational to do, judge the appropri-

ateness of a man's behavior.

Whereas a scientific law is something we discover or presuppose, a principle of action is a standard by which we measure behavior. We would lose confidence in Carr's explanation of Marx's behavior if we believed that Marx did not really think that the Bakuninists were trying to take over the International but really thought that the International would best serve his own purpose if the General Council remained in London. For in these circumstances we could not imagine any reasonable person making the proposal that Marx made. We should have to turn to another type of explanation: psycho-analytic, perhaps.

I have said that in a Dravian explanation a principle of action serves the same function that a general hypothesis, or law, serves in a Hempelian explanation. It gives us confidence in an explanation. But what is the logical justification for the confidence? In Hempel's first model of explanation the confidence we have in an explanation is an expression of the fact that the explanandum is brought under covering laws. The deducibility requirement is met. Similarly, the deducibility requirement is met, or supposed to be met, by explanations

which conform to Scriven's model.

Does a Dravian explanation meet the deducibility requirement? In an attempt to answer this question I shall give what I take to be the form of a Dravian explanation:

1. When in a situation of type C the thing to do is x.
2. Agent A is in a situation of type C.
3. Therefore, the thing for agent A to do is x.
4. Agent A does the thing to do (i.e., Agent A is rational).
5. Therefore, Agent A does x.

The first premise is the principle of action. The second premise is a factual premise; in the context of history its substitution instances would be historical statements about the circumstances in which historically significant people found themselves. The third premise is a derivation from the first two. The fourth premise reflects a standing presupposition of a Dravian explanation. We are to presume that an agent is rational until he is proven to be otherwise. We need not assume that he is always rational but if the posit that he is rational

fits in with an attempted explanation of his behavior we are justified in making it. The conclusion follows from the third and fourth premises. If this proposed reconstruction of a Dravian explanation is correct, it serves to answer Passmore's remark that "explanation by reference to a 'principle of action' or a 'good reason' is not, by itself, explanation at all."¹¹ Passmore believes that a Dravian explanation only shows us that what the agent did was the thing to do but fails to show us that the agent did what he did because it was the thing to do. He overlooks the fourth premise of a Dravian explanation. We can only give a rational explanation of Marx's behavior if we assume that Marx was rational; if we do not believe that Marx would act on what were good reasons from Marx's own point of view, there is no point in looking for these good reasons unless we want to show that Marx, in fact, behaved foolishly or insanely.

Professor Hempel does not overlook the fourth premise but he argues that any explanation pattern which

¹¹John Passmore, "Review Article: Law and Explanation in History," Australian Journal of Politics and History, Vol. 4, 1958, p. 275.

includes it is actually a covering law explanation in disguise:

When modified accordingly, our explanans takes on a form which may be schematized as follows:

Agent A was in a situation of kind C. A was a rational agent at the time Any rational agent, when in a situation of kind C, will invariably (or: with high probability) do X,

and it will then logically imply (or confer a high inductive probability on) the explanandum:

A did X ¹²

Hempel replaces "Dray's evaluative principle of action by a descriptive principle stating what rational agents will do in situations of kind C."¹³ Hempel's line of reasoning is this. If I show that what an agent did was the appropriate thing to do, given the agent's circumstances as the agent understood these, and imply that he

¹²C. G. Hempel, "Reasons and Covering Laws in Historical Explanations," in Sidney Hook, ed., Philosophy and History (New York University Press, New York, 1963), p. 155. In my edition, there is no punctuation after "time" in the sixth line nor at the end of the logical implication.

¹³Ibid., p. 155.

did what he did because he was rational I commit myself to the generalization that he did what any rational agent would have done in sufficiently similar circumstances. If I reject the generalization--which Hempel treats as a covering law--then I must revoke the implication that he did the appropriate thing because he was rational. If being rational is no guarantee that one--if free to do so--will perform the appropriate action the posit of the agent's rationality will not serve to explain why the agent did what he should have done. We should have to look for another link between the knowledge that the agent should have done a certain thing and the fact that he did it.

Hempel deals with the objection that his "covering law" is actually an analytic statement to the effect that "rational agent" means "one who does appropriate things."¹⁴ If this were so, the "covering law" could not serve as a universal hypothesis since it would have the logical status of a definition.

¹⁴Ibid., p. 156. The above formulation is not Hempel's. I have used quotation marks to distinguish the definiendum from the definiens.

If I understand him correctly, his argument rests on the logical point that an analytic proposition can have no empirical consequences. But when we say that a man is rational, we imply that he will respond to stimuli in various ways. If he does not do this, we withdraw the claim that he is rational. The claim--as I understand Hempel--is in the nature of a hypothesis about the man; a hypothesis which adverse empirical results can falsify. "Rational agent" is a concept which rests upon a theory of what it is to be rational, rather than a definition of what it is to be rational. The general statement that any rational agent, when in a situation of kind C, will invariably (or: with high probability) do the appropriate thing (Hempel incorporates "appropriateness" into his transformation of Dray's principle of action although this fact does not show up in the abstract scheme of Hempel's transformation) is what Hempel calls a "symptom statement."¹⁵ It states one of the ways in which a disposition is always, or likely, to be realized. It is an empirical proposition forming part of a theory of rationality and the concept

¹⁵Ibid., p. 156.

"rational agent" is a dispositional concept of a theory-laden type.

Hempel's position on this point seems to me unsatisfactory. If we grant, with Donagan, that the historian tries to show us why a particular agent did what he did, a probabilistic covering law will not do. If, however, we treat Hempel's covering law as a universal hypothesis, we run into difficulties. If we encounter a seeming counter-instance to the law, we may deal with it in one of two ways: (a) we can give up the law or (b) we can try to show that the counter-instance was, in fact, only a seeming counter-instance. If we opt for "(b)," there are, again, two things we can do. We can simply say that any behavior which is at variance with the law cannot be rational behavior and this procedure--by ruling out the possibility of falsification--empties the "law" of empirical content. The other alternative is that of stating the range of conditions under which the law is supposed to hold. Some of these conditions--or likely conditions--are easy enough to state. We would not treat it as an infirmation of the law if we found out that a rational agent was prevented from doing the appropriate thing because, say, the

N.K.V.D. had broken in the door before he had time to set fire to a message from Trotsky. He knew what he should do and would have done it (destroyed a damaging communication) if he had not been prevented. However counterfactual conditionals are to be analyzed, there is no doubt that they play a role in saving laws from infirmation.

But it is impossible to show the range of conditions under which Hempel's law is supposed to operate even if certain presupposed conditions can be taken for granted. Consider Carr's explanation of Marx's action. Filled in, Hempel's general law would read: "Any rational agent, when in a situation of the kind Marx was in, will do the kind of thing Marx did." (I have dropped out the probabilistic variant.) I have--to make Hempel's law more plausible--weakened the predictive scope of the law to "kind of thing." A "general law," which stated that a rational agent, in a situation of the type Marx was in, will do precisely what Marx did, would either be absurd (because only Marx could satisfy that requirement) or a covert repetition of the case which was supposed to come under

it.¹⁶

Is Carr committed to Hempel's general law? It is really implausible to suppose that he is. Other people have been in situations similar to that of Marx without doing anything like what Marx did. Even if we presuppose that they are rational at the time they take action--assume that they will act on their assessment of the situation--it does not follow that they will do what Marx did. It is always possible, and very likely, that their circumstances will not be sufficiently close to those in which Marx found himself to allow the prediction that they will do what Marx did. The "general law" might well have to suffer, in Anthony Flew's well-known phrase, "the death by a thousand qualifications."

If Marx's action was rational, a "general law" covering Marx's case either does not exist or does not explain the connection between Marx's reasons and Marx's action. I shall try to clarify this point. Hempel, in converting Dray's principle of action into a descriptive principle, commits himself to the proposition that that

¹⁶I am borrowing an argument from Dray, op. cit., pp. 31-57.

descriptive principle is a covering law. But a covering law which we see no way to apply is as good as no law at all. Carr could have no use for Hempel's covering law because he could not use it as a principle of inference. When he judges that Marx's proposal made sense, he does commit himself to a principle of action. He does commit himself to the proposition that what would be a good reason for Marx would be a good reason for anyone in Marx's position (anyone having goals like those of Marx and facing circumstances such as those Marx faced). But he does not commit himself to the proposition that anyone has ever been in Marx's position nor does he commit himself to the proposition that if anyone ever were in Marx's position he would do what Marx did. He seems to commit himself to the proposition that if any rational person were in the position Marx was in, he would do the type of thing Marx did.

But this appearance is deceptive. Carr presumes that Marx is rational. The evidence in favor of the presumption is, in effect, the demonstration that what Marx did was the thing to do. On Dray's model we are entitled to assume that an agent is rational unless there is evidence to the contrary. A successful Dravian

explanation, among other things, rebuts the notion that there is evidence to the contrary in a specific case. But to see that Marx did the thing to do is not the same as to see that he did it because he had a disposition to act rationally. Carr presupposes that Marx was rational at the time of the Council meeting and this simply means that Carr supposes that Marx would do the sensible thing. What I have listed as the fourth premise of a Dravian explanation makes this clear. The assumption that the agent is rational is the same as the assumption that the agent does the thing to do. It is not a law-like assumption in which rationality pushes one into doing appropriate things. It may be that we are conditioned to do appropriate things (I would think that this was true of the vast majority of our actions although this is not the place to support my beliefs on the matter) and, possibly, the fact that a man is rational can be explained by covering laws.¹⁷

But Carr is not trying to show why Marx was rational but why what Marx did was a rational thing to do.

¹⁷See William Dray's "The Historical Explanation of Actions Reconsidered," in Hook, op. cit., p. 115.

Carr is committed to Dray's principle of action in the form: "A person who did the sort of thing Marx did in a situation similar to that of Marx for the kind of reasons Marx had would be a rational person." The principle of action fits the case but it is not an empirical law covering the case.

We are now in a position to return to the question of whether or not a Dravian explanation meets the deducibility requirement. If we reject Hempel's reformulation of Dray's model we abandon the idea that an empirical law licenses the inference that the agent did what he did because he was rational and what he did was the rational thing to do.

As I have formulated the model of a Dravian explanation it appears that any fill-in of the model would meet the deducibility requirement. But before this conclusion can be drawn we must consider the objection that Professor Dray does not intend that his model meet the deducibility requirement and that, in fact, I have misconstrued the model:

For although a rational explanation does not show that a particular action had to occur, it may show (in a rational sense

of "had to") that a particular action had to be done. I say only "may" because I do not think that the puzzlement which leads a person to ask why someone did something is, even in the rational sense, always expressible as "Why did he have to do that?" Sometimes all we really want to know, when we ask for an action to be explained, is how it could have seemed rationally possible or "all right" to do it; and here an issue like that between deductive and inductive approaches to the covering law model arises within the class of rational explanations. When we ask why an action had to be done, however, an answer bearing directly on the case is often possible--an answer involving the rational ruling out of all possible alternatives. There is no counterpart of this in the assertion of a purely statistical connection.¹⁸

It is clear, I think, that Professor Dray would reject my reconstruction of a Dravian explanation if the conclusion of the reconstruction, "Therefore, Agent A does x" had to be equated with, "Therefore, Agent A necessarily does x." But although the conclusion is a logical consequence of its premises (its denial entails the denial of one or more of them under pain of contradiction) this does not justify the importation of a modal operator into the conclusion itself. The modal operator

¹⁸Ibid., p. 120.

would have to be contained in the premises to justify its inclusion in the final line of the argument form.

The point is this. To say that an agent does x for a reason is not to say that he had to do x in a law-like sense of "had to." It may be that, if we are able to eliminate all rational alternatives, we can say that a rational agent had to do x in a rational sense of "had to." My model of a Dravian explanation is of the form in which it is assumed that one course of action is preferable to any other from the agent's point of view. When there are, say, two alternatives which are equally sensible from the agent's point of view and conjointly exhaust the range of "good reasons," we can modify the model in this way:

1. When in a situation of type C the thing to do is x or y and it is false that x = y.
2. Agent A is in a situation of type C.
3. Therefore, the thing for agent A to do is x or y.
4. Agent A does the thing to do.
5. Agent A does not do x.

6. Therefore, agent A does y. (Or agent A does x if the fifth premise is false.)

I have not tried to formulate a Dravian model for those cases where we are only concerned to know that an agent could have had a reason for what he did and are not concerned to know whether or not it was the best reason from the agent's point of view. But could I rationally understand the agent's action if I did not presume that he thought it was the best thing to do under the circumstances? Suppose I found out that he thought that there was a better thing to do, which he was free to do, but chose the lesser good over the greater good. Would I not be inclined to believe that he had not acted rationally? I do believe that there is a distinction to be drawn within the class of rational explanations but I do not believe that it is the distinction Professor Dray seems to draw. There is a distinction between those Dravian explanations which presuppose no equally rational alternatives and those which do. The latter can be put into deductive form by adding disjunctions to the principle of action premise and eliminating all of the disjunctions but one (the conclusion serves to do this since it negates all but one of the

alternatives).

It may be that Professor Dray is simply saying that there may be more than one perfectly good reason for an action and, where this is so, there is no point in asking why the agent chose to act in accord with one of those perfectly good reasons (given freedom of choice but not of conjoint satisfaction) rather than the other.

My "alternatives model" does not tell us why one rather than the other; it only tells us that if not one then the other. The conclusion of the citation from Professor Dray's remarks suggests that he may have had this in mind.

A Dravian model which does not allow for alternatives shows us that a rational agent had to do what he did in the rational sense of "had to." A Dravian model which allows for equally rational alternatives shows that a rational agent had to do what he did, in choosing one of these alternatives rather than by-passing all of them, in the rational sense of "had to."

The rational sense of "had to" is to be distinguished from the causal necessitation sense of "had to." A Dravian "reason" is not an event which invades a man as

pollen may invade my nose and causes him to do what he does in the way that the pollen causes me to sneeze. A reason is something that a man has. A man acts on a reason, the reason does not act on him. (I am not denying the obvious truth that "reason" and "cause" are often used interchangeably; but this does not alter the fact that they are often used differently and it is the different uses which are relevant here.)

In other words, it makes perfectly good sense to say that a man had to do what he did but was free to do it. He had to do what he did because it was the only rational thing to do, but he was free to do it in the sense that his reason for doing it was not the same as a cause compelling it. It may be that some things a man does come under the rubrics of both rational and covering law explanations. I may act from a reason but be caused to act as I do because of my conditioning.

It now seems as if we are in a position to conclude that a Dravian explanation meets the deducibility requirement. But there is a final objection to be considered. My formulation of a Dravian explanation is highly artificial. The formulation gives the impression that, in

practice, a Dravian explanation has a deductive form and that this is a very important fact. But it is not really a very important fact from a practical perspective. The principle of action is only in a very weak sense a principle of inference. It is better to say that the historian commits himself to a principle of action (which it may be difficult if not impossible to express in a complicated case¹⁹) than that he assumes a principle of action.

When we speak of the logic of a Dravian explanation it is understandable that we should emphasize the role of the principle of action for this is the only general proposition we have to work with in comparing a Dravian explanation with a Hempelian explanation of the covering law form. But, in practice, a principle of action may grow out of a specific analysis rather than presuppose the analysis. For example, Carr and Berlin may not have thought about any principle of action at all. The important thing is that they saw that Marx had good reasons for what he did. They committed themselves to a principle of action

¹⁹Dray, Laws and Explanation in History, pp. 132 and 171, Note D.

but they were not necessarily guided by a principle of action nor did they necessarily presuppose one. It is when we seek out the form of an explanation that we are likely to make too much of the principle of action or even, as with Hempel, think of it as a disguised covering law. Nonetheless, considered from a purely formal standpoint, I think we may say that a Dravian explanation meets the deducibility requirement. The "alternatives model" meets the requirement by what may seem to some a bit of sleight-of-hand on my part. But there is really no sleight-of-hand. If we decide that what an agent did was as reasonable as anything else he could have done (and admit that other courses of action would have been equally reasonable) but wonder why he did it rather than one of the other equally rational alternatives, the model will not be of any use. (I mean, of course, any specific example of the model will be of no use.)

I think that Dravian explanations of the kind I have been concerned with in this chapter are exemplified in historical writings; that, for example, the discussion of Marx by Carr and by Berlin is of the Dravian type. There is, however, the question of whether or not a Dravian

explanation (of the type, as always in this chapter, in which why questions are answered) is the only counter to Hempelian explanations within the domain of historical writing.

Alan Donagan has argued that there is a counter to Hempel's model which (a) meets the deducibility requirement or, in Donagan's language, satisfies the "deductive thesis" and (b) is non-Dravian. His model, which I shall give below as formulated in a recent article, has been developed as a result of his reading of R. G. Collingwood, but I shall discuss it independently of the context of its development.

Donagan gives this schema of one allegedly non-Dravian non-Hempelian historical explanation;

A was resolved to achieve the end E
at all costs

A judged his situation to be C

A judged that E could only be achieved in C
if he did x

Therefore, A did x.²⁰

²⁰Donagan, op. cit., p. 24.

It will be useful to fill in the schema by means of the example that Donagan used as the basis for the schema.²¹ I am not sure that the fill-in I shall provide for the second premise will prove to be the one Donagan had in mind (he does not fill in the schema) but it should prove satisfactory for the purposes of the present discussion:

Brutus was resolved to preserve the Republic at all costs.

Brutus judged that the Republic was in danger.

Brutus judged that the Republic could only be preserved if he joined Cassius' conspiracy.

Therefore, Brutus joined Cassius' conspiracy.

Professor Donagan argues that although his explanation satisfies the deductive thesis without recourse to covering laws it is not a rational explanation. The explanation, according to Donagan, does not presuppose that either Brutus' resolution or his judgments were "rational"; for it does not evaluate Brutus' thoughts in

²¹Ibid., p. 20.

any way at all.²² He implies that this is an advantage over a Dravian explanation. He seems to think that a Dravian explanation is defective on two counts and that his own explanation avoids these defects. The first defect relates to the principle of action. Donagan states that "there is no criterion of rationality which uniquely singles out one course of action as 'the thing to do.'"²³ I do not think that Donagan's objection is sound; firstly, it overlooks the judgmental aspect of a Dravian explanation. It is the historian, trying to find out what the agent made of the situation in which he found himself, who concludes that the agent acted for the best reason (the best reason, that is, from the agent's standpoint) and commits himself, at least implicitly, to the proposition that the agent's reason, or reasons, would be good reasons for anyone else in sufficiently similar circumstances. He may be mistaken about this. Secondly, since the

²²Ibid., p. 24. The material from "presuppose," on line 16, p. 193, to "all," on line 1, p. 194, would be in direct quotation marks if it were not for the fact that I have replaced Donagan's "dummy-constants" A, C, and x by constants.

²³Ibid., p. 24.

historian's goal--within a Dravian framework of explanation--is to find out what were the best reasons from the agent's own standpoint, it seems odd to suppose that the agent's way of thinking does not ever lead to a course of action which is viewed as "the thing to do." If the historian does not find that the agent's thinking was defective, why may the historian not conclude (in cases where there is no rational alternative for the agent, given the agent's goals and understanding of his situation) that the agent did "the thing to do"? If it be said that there are no such cases, this seems to be the sheerest dogmatism. Surely we sometimes are in situations where one course of action is the best for us, given the information at our disposal. We have no a priori reason for saying that an historian--if any of our actions should ever become of historical interest--could not understand why we acted as we did.

Donagan proposes that a Dravian explanation (if Hempel's reconstruction of a Dravian explanation is correct) also suffers from the defect of supposing that

all historical agents are rational.²⁴ This is a difficult objection to understand. Donagan holds, I take it, that the propositional function "x is an historical agent and x is not rational" has true substitution instances. Let x = A and the proposition replacing the above propositional function have the truth-value true. In this case, the Hempelian form of a Dravian explanation will not work; since that form gives us:

A was in a situation of type C.

A was a rational agent

In a situation of type C a rational agent
will do x.

Therefore, A did x.

The denial of the second premise is compatible with the denial of the conclusion. But this is hardly a defect of the model. The model would be useless if the propositional function "x is a rational agent" always had false substitution instances. But Donagan does not say, or imply, this. The point I am making here applies to Dray's original model as well as to Hempel's reformulation

²⁴Ibid., p. 24.

of it. A Dravian explanation works when it correctly presupposes that the agent was rational; a priori, unless the propositions replacing the propositional function "x is a rational agent" are always false, it is impossible to show that the presupposition is always mistaken. Therefore, we cannot say that a Dravian explanation can never be satisfactory.

If, on the other hand, we accepted Donagan's first objection, we would be forced to conclude that a Dravian explanation can never be satisfactory: for if no substitution instance of the form, "In a situation of Type C a rational agent will do x," is ever true, then there is no such thing as a Dravian explanation in Hempelian form. But, even if we reject Hempel's formulation of Dray's model as I think we should, Donagan's point, if accepted, would apply to the original model as well. If there is no way of discerning that one course of action is best, then the principle of action, "When in a situation of type C the thing to do is x," can never be justified. One cannot show that the agent did the thing to do if one can never know what it is to do the appropriate thing. I have already criticized this objection of Donagan's but I bring

it up again because it leads us to see what he has against Dravian explanations.

Donagan's major objection to Dray's position is that he thinks Dray's position entails that an historian can understand only rational actions. Therefore, even if Dravian explanations are sometimes successful (and if Donagan will not give up his first objection I do not see how he could concede that Dravian explanations were ever successful) they limit the historian.

Actually Dray's position entails only that a rational explanation is possible if and only if it displays the rationale behind a human action. "We give reasons if we can, and turn to empirical laws if we must"²⁵ is a remark of Dray's which calls attention to the fact that we may, after all, be forced to explain some human actions in terms of empirical laws. In these cases, however many or few of them there are to concern the historian, we give law-like explanations rather than rational ones. I think, however, that Donagan's mistake rests upon a genuine disagreement. Even if Dray admits that historical explanations

²⁵Dray, Laws and Explanation in History, p. 138.

may sometimes be causal or law-like rather than rational ("causal" in the Hempelian sense where a covering law is involved, or presumed to be involved), he may nonetheless suppose that to explain why someone did something is either to give a reason for the action or to list a cause, or causes, of it. But Donagan supposes that there are explanations which are neither covering-law causal explanations nor rational explanations.

An example of this type of explanation is alleged to be Donagan's explanation of Brutus' action. We are now in a position to see why this is an important example. Whether or not Dray's model is defensible against Donagan's objections, Donagan is quite sure that his own example of a non-Hempelian, non-Dravian explanation is safe from these objections. I think that Donagan is right in holding that his explanation is non-Hempelian. But is it non-Dravian? Donagan says that his explanation does not evaluate Brutus' thought in any way at all. But does it not? Suppose that the first three premises are true? The conclusion is alleged to follow from these. But it seems to me that the falsity of the conclusion is compatible with the truth of the premises. If Brutus were insane he could, so far as I

can see, be resolved to preserve the Republic at all costs and judge that it could be preserved only if he joined Cassius' conspiracy and yet fail to join it. He might fail to see the rational connection between judging that he should join the conspiracy and actually joining it. This would be, I am sure, a very rare form of insanity but it is not, surely a logically impossible form of insanity. Donagan must posit, implicitly, that Brutus was rational enough to act on his judgments. This turns Donagan's explanation into a rational one. Brutus joined the conspiracy because he reasoned that this was the best way, or the only way, to preserve the Republic. If Brutus did not reason at all, we could have no confidence in an explanation that told us that he acted on his judgments. We would wonder why he acted on them.

Donagan says that "if a man has refrained from a certain act which he judges to be necessary to a certain end, it immediately follows that, when he refrained, he was not resolved to achieve that end at all costs. He might once have been so resolved, and have lost his

resolution: but that is something else."²⁶ But suppose that Brutus lost his nerve? Can we not imagine Brutus, resolved at all costs to save the Republic, trying to screw up his courage to join Cassius' conspiracy? Could not his judgment exceed his capacity for action? Surely Brutus, by the terms of the explanation, must not only be sane enough to see a rational connection between his judgment and his action but he must also be sane enough to make judgments which he has the right to believe could possibly be carried through into actions. The historian is, after all, assuming that Brutus is a rational agent. But, even if the reader agrees with me on this point, it may be said that there is no principle of action to be extracted from Donagan's example. In this sense, at least, a Donaganian explanation, although to some degree a rational one, need not be a Dravian explanation. After all, Brutus may have reasoned very poorly even if it be admitted that it must be supposed that he did reason in some fashion or other.

Are we entitled to infer that any actual agent,

²⁶Donagan, op. cit., p. 20.

with Brutus' goals and in the circumstances in which Brutus actually was, would reason as Brutus did? The answer to this is no. We are entitled to infer, or commit ourselves to, only the proposition that any rational agent who judged things as did Brutus would act in the way that Brutus acted. If Brutus' judgment is so defective that the historian cannot follow it, the historian could have no confidence in asserting that Brutus resolved such-and-such or that he judged he ought to do so-and-so.²⁷ A Dravian explanation does not preclude the possibility that an agent acts on peculiar beliefs²⁸ but there is a point at which a rational understanding of a man's behavior must be given up. When we cannot give reasons, we turn to laws or assume their presence. If Brutus did not have a good reason enjoining his action--if only a good reason from the point of view of one with very peculiar beliefs--we either treat Brutus' action in joining the conspiracy as a brute, inexplicable fact or, perhaps, we look for a psychological

²⁷Dray, "The Historical Explanation of Actions Reconsidered," see pp. 112-3.

²⁸Dray, Laws and Explanation in History, p. 137.

explanation.

So far, in this chapter, I have attempted to describe the properties of a non-Hempelien model of explanation which is exemplified in history. I have not attempted a complete survey of the objections to the model but I believe that the arguments against it (including under arguments against it Hempel's attempt to show that it is a defective covering law model) which I have considered are central. Nor--by offering objections to Donagan's example of a non-Hempelien, non-Dravian model--have I shown that Dravian explanations are the only alternative to Hempelian explanations. Even if my objections to Donagan's example are sound, this says something only about the example and whatever generalization it is alleged to support. In fact, other models of explanation than those considered in this, and in the preceding, chapter have been proposed. But even if there are other sound models of explanation, it is enough, for the general purposes of this dissertation, to deal with the models which have been considered.

For if I am correct in arguing that both Hempelian and Dravian explanations have a place in historical research--if the reader thinks that my analysis and

examples support my contention satisfactorily--we have an answer to the question of the relationship between history and the applied sciences vis-a-vis the question of the role of laws.

It is clear, if my contentions are correct, that history, unlike an applied science, makes use of explanations which do not consume laws or presuppose them. It is also clear that some historical explanations do presuppose laws in the sense that an historian will assume that the agent's behavior is of a type covering laws would explain --whether or not the historian is in any position to know what those laws might be. There is a gap between history and the applied sciences even here; for the applied scientist knows what laws he is applying; he could hardly settle, in his own line of work, for an assumption that was in the nature of a blank check.

But we are left with the question: should one pattern of explanation be the ideal pattern in history? Should the historian concentrate on giving us one kind of explanation or favor one type over the other? In the following chapter I shall consider this question.

CHAPTER VIII

HISTORICAL PERSPECTIVES

Analysts have, with few exceptions, treated the models of historical explanation which we have considered as competitive; Collingwood held that his model of historical explanation was incompatible with a covering law model --he did not believe that the historian, qua historian, could, in different connections, make use of both types of explanation. Hempel tried to show that the model of rational explanation, as developed by Dray, is reducible to the covering law model--thus attempting the liquidation of the competition through its absorption.

Critical philosophers of history, whether or not "positivistic," usually maintain that the methodology of history rests, or ought to rest, upon some one explanatory pattern; some one model of explanation is usually treated as the right reconstruction of the historian's explanatory

narratives.

How do we account for the dogmatism exhibited by and the persistence (well over a century) of the dispute? What has made it possible for the "positivists" and their opponents to carry on a dialogue for over one hundred years without any sign of a "coming to terms" in sight?

One answer to these questions, which I suggest as an hypothesis, is that the philosophers of history conducting the debate have been philosophers of different histories. They have not meant quite the same thing by "history" although the persistence of the debate testifies to the fact that they have meant similar things by the term "history."

I do not mean to imply, in suggesting this, that the dispute has been a simple "semantic dispute" which could have been settled if only some G. E. Moore had said to the disputants: "But what, precisely, do you mean by the word 'history'?" This disputants might, conceivably, give the same answer to our hypothetical G. E. Moore and remain as far apart as ever. It is fairly easy to see how this situation would arise.

This would happen if they did not go far enough

in answering the question; if they did not mention not only what the word meant but also what the word ought to mean. When I suggest that the disputants--the positivists and their opponents--have not "meant quite the same thing by 'history,'" I do not mean that they would not refer to to the same sorts of books or mention the same kind of content. They may well do this and yet "not talk the same language." The disputants themselves may not be able to separate out what they refer to when they talk about "history" and how they evaluate what they call history.

They may agree about what is to be described as historical practice and disagree about the ideals supposedly behind that practice--the ideals the historian must be striving toward. Part of the meaning of the word "history" for these disputants is a stipulation about what history ought to be; this stipulation controls analysis because what one picks out of history--the examples and procedures one chooses--are selected in accord with the stipulation. An "historical perspective" or "metaphysics of history" is brought to bear on the data of history. This interpretive framework is not obvious because it is compatible with what has been selected and what has been selected is certainly

a part of history.

An "historical perspective," as I shall use the phrase, is an implicit or explicit classification of the total narrative pattern of what the classifier takes to be "history." This classification is in the nature of an "outlook" about the structure of history; what is, in general, the nature of historical exposition. Detailed examples of such "historical perspectives," and how these perspectives fit in with the dispute between the "positivists" and, as their opponents are sometimes called, the "idealists," will further clarify the meaning of "historical perspective" and show the importance of the topic in connection with the problem of the nature of history.

Lee Benson and Cushing Strout, both historians, have written a joint article on the problem of giving adequate explanations (or any explanation at all) of why the American Civil War came about.¹ In the course of this article--really two articles without interchange of opinion--each clearly articulates an historical perspective

¹Lee Benson and Cushing Strout, "Causation and the American Civil War," History and Theory, Vol. 1, No. 2, 1961, pp. 163-85.

which consciously guides his interpretation of the nature of historical explanation.

Both maintain that historical writings, in general, resemble literary art-forms and would subscribe to significant parts of Gallie's analysis of the nature of historical narrative. Lee Benson argues that the literary art-form closest to history is that of the novel or, more precisely, the novel as understood by E. M. Forster, in his Aspects of the Novel. According to Benson:

Although historians also use the narrative device, "and then," to tell what happened to men over time, they aim to do more than tell a story or present a chronicle of events. For a history and a chronicle differ in essentially the same manner that a plot and a story differ in the novel. To quote Forster:

"Let us define a plot. We have defined a story as a narrative of events arranged in their time-sequence. A plot is also a narrative of events, the emphasis falling on causality. 'The king died and then the queen died' is a story. 'The king died and then the queen died of grief' is a plot. The time-sequence is preserved, but the sense of causality overshadows it . . . Consider the death of the queen. If it is a story we say 'and then'? If it is in a plot we ask 'why?' That is the fundamental difference between these two aspects of the novel."

Using Forster's criterion, we can define a historian as a plot-teller. Unlike the chronicler, the historian tries to solve the mystery of why human events occurred in a particular time-sequence. His ultimate goal is to uncover and illuminate the motives of human beings acting in particular situations, and, thus, help men to understand themselves.²

On Forster's view, which Benson accepts without qualification, the novelist makes "cause and effect" the principle of connection for incidents in a novel. The novel is seen as a work of plausible fiction. The actions of the characters are subsumable under covering laws. This does not mean that the novelist may not create his own world--like a fantasist or science fiction writer--in which the "laws" are different from those of the world in which we live; it means that, whatever world he creates, the characters in that world will behave in explicable ways. Even, to take an example from Forster's own work, the locomotive which does away with Howard, at a convenient moment in Howard's End, does not offend against possibility; the train is not--I take it--a symbol of brute, absurd, contingency; although it may be taken as a sign of

²Ibid., p. 163.

flagging inspiration on the part of E. M. Forster.

The novelist need not state his "laws," but the reader must be able to infer or "intuit" them in order to be convinced that the characters are "plausible" and the events brought about by their behavior "hang together." Generally, the novelist accepts, as a presupposition, the science of his day as valid and writes novels in which that science is not challenged. In this case the characters must be plausible in the light of that science; i.e., they must be believable; their psychological motivations must make sense--the novelist must avoid "foreshadowing" which would make a character's later reactions simply unbelievable.

I do not think that Forster's claim that the novelist must make use of a principle of causality is correct. As I have said previously, I do not see how that principle is, at all times, invoked in Olesha's Envy. Nor, to take an example of a novel which Forster, perhaps significantly, despised, do I see that Joyce's principle of organization in Ulysses is always that of "causality."

(In this chapter I always have reference to the scientific use of "cause," "causality," or "cause-and-

effect." This fits in with Benson's use of the term.) Certainly Stephen Dedalus and Leopold Bloom can be understood, but I cannot imagine what laws there are supposed to be from which this understanding might be alleged to follow. No doubt much of the content of Bloom's mind is linked together by principles of association but these principles are not exhaustive in their scope. They apply but they do not permit us to anticipate a given association at a given time; nor are we entitled to infer that Bloom's mind is rather like a deck of cards in which many different hands are possible but the number of cards remains constant. Even he has new ideas which do not come from the old deck. The principles of association, which Joyce seems to have taken over from Aristotle and Freud, apply only "more or less." However, Forster could reply that novels which do not satisfy his criterion are "disorganized" or "flawed" to the degree to which they fail to satisfy the criterion. It is possible that he would say this of Andre Breton's Nadja, Jean-Paul Sartre's Nausea, Samuel Beckett's Murphy, or Robbe-Grillet's Voyeur as well as James Joyce's Ulysses.

Benson, while accepting Forster's analysis of the

novel and thinking the analysis applicable to works of history, recognizes of course that the novelist has an advantage over the historian in that he creates his characters and can assure himself that they behave as they ought to behave; he gives them their "biography" and knows all there is to know about them. The historian, on the other hand, can not fill in gaps by making up "facts" and this, very often, entails giving up the hope of explaining an agent's behavior.

But if the historian were to explain an agent's behavior it would be, on Benson's view, in accord with the principle of causality because it is this principle which is behind the very possibility of organizing history into a coherent whole. To the extent to which this principle is inapplicable we do not have history at all--but only chronicle, the noting of a succession of events:

To do his job successfully, the historian has to assume the existence of general laws of causal dependence. That is, he has to adhere to certain logical principles (or laws) which, I suggest, govern any explanation of human behavior, whether the attempt finds expression in a poem, a novel, or a historical mono-

graph.³ [The italics are mine.]

What organizes a novel is what organizes history. The comparative lack of "finish" of a work of history is testimony to the fact that the historian is not in the enviable position of the novelist; he must show not only what can happen but also what did happen, and why it happened, insofar as his knowledge permits an explanation of what he depicts.

Benson's "historical perspective" is his explicit position that what makes the discipline of history possible, as an explanatory discipline, is the use of the principle of causality. History is, at least in tendency, a science because the use of this principle is combined with the quest for truth. History may be mistaken for an art because, in many respects, it has the form of an art; i.e., the "story" or the "novel." But its desired content is fact and its methodology is scientific; the methodology rests on the causal principle, which is common to the historian and the novelist, but this principle is also common to the historian and the practitioners of other

³Ibid., pp. 163-4.

sciences. If we considered the use of this principle as, per se, the hallmark of science, we could not methodologically distinguish the novel from a treatise in physics. It is the commitment to "what actually happened"--the historian's form of scientific empiricism--which differentiates history from the narrative art-forms which it otherwise resembles so closely.

This historical perspective, when contrasted with that of Cushing Strout, may bring out the undercurrents of the epistemological/metaphysical dispute between the "positivists" and the "idealists." Cushing Strout agrees with Benson that history, in its form, resembles the literary arts. But the art-form which he thinks closest to it is that of the drama. Strout has what I will call a "dramaturgical perspective of history." The organizing principle behind the development of genuine historical narratives is not that of causality:

The causal problem would be greatly clarified if both historians and philosophers realized that in telling a story the historian is committed to the "logic" of drama. In explaining the Civil War he necessarily seeks to recreate the strife of opposing forces out of which the war came. The connective tissue of his account then has a dialectical form: a

person or group takes a position and performs an action because of and in relation to the position or action of another person or group. The historian's story becomes a narrative of this reciprocal response. Thus, by a crude sketch, the explanation of the event would have this character: Lincoln saw in the South's pro-slavery position a threat to the democratic traditions of the American community; the South saw in his election the menace of future interference with their "peculiar institution" and growing domination by an industrial North; Lincoln and the North saw in Southern secession a challenge to federal authority and the prestige of national union; the South saw in the provisioning of Fort Sumter an intolerable danger to independence of the Confederacy . . . In such terms, but with much greater richness and concreteness, the historian tries to reconstruct the dramatic "logic" of a sequence of events which demands to be humanly understood rather than scientifically explained.⁴

The historian, like the dramatist or the novelist, has a story to work from; some set of human actions which have contributed to, or are responsible for, the events in which he is interested. But the connective tissue of his account exhibits a dialectical form:

This dialectical method does not entail any Hegelian scheme or "bloodless dance of the categories"; on the contrary, it

⁴Ibid., p. 184.

keeps the historian in touch with the familiar existential world of human action, too concrete and passionate for final abstract accounting. Like the action of a novel or play, it can be imaginatively experienced as a meaningful plot in which character, events, and circumstances are woven together in a process made intelligible in human terms of tradition, interest, passion, purpose, and policy. . . . To ask the question "why?" is then meaningful only as a demand for enlightenment on some particular passage of the story which does not "make sense." The general causal question remains at worst an irrelevant basis for interminable disagreement, at best a generator of hypotheses to stimulate research which may promote understanding by leading to a richer, more coherent story.⁵

An example, which is mine rather than Strout's, will bring out the nature of the "dialectical method" as Strout sees that method incorporated in history.

In Hamlet, Shakespeare presents us with the well-known scene in which Hamlet has the opportunity to kill the king. The king is unguarded and, being at prayers, is in a defenseless position against any sudden onslaught. Hamlet, in his own mind, has ample justification for killing the king and has no scruples about regicide. But he

⁵Ibid., p. 184.

draws back after overhearing the king seemingly repent of his crimes. After all, it is, on Hamlet's view, no real revenge upon a man to dispatch him to Heaven where he will be better off than ever! The fitting revenge, for the king's own double crime of regicide and fratricide, is that which both severs the soul from the body and sends the soul screaming into a hell which will be its place of suffering for all eternity.

After Hamlet leaves the scene it is brought out, in the king's monologue, that there was no genuine repentence at all. Had Hamlet been on the scene a little longer Shakespeare's play would have continued as the history of the regicide, Hamlet. As it happens, this segment of the play is usually chosen in illustration of the character of Hamlet, the "man who could not make up his mind." But the segment can also be used to illustrate a dramaturgical principle.

Until Hamlet does turn away the audience--any audience, that is, which is unfamiliar with the play--does not know whether Hamlet will kill the king. Certainly, if Shakespeare had so written the scene that Hamlet does kill the king, a play could go on. The audience does not know

whether Hamlet will kill the king or withdraw because either action would make sense. By killing the king, Hamlet does satisfy the requirement that a usurper be removed from the throne. He does all that he must do--as his father's vehicle of vengeance--in taking an eye for an eye. Also, by that act, he removes Denmark from its curse--a curse underscored by ill-omens mentioned in the play. Moreover, he cannot be sure that he will ever again have such a good opportunity; the heightening of the king's suspicions may lead to Hamlet's removal or liquidation and will certainly lead to greater caution on the part of the king; prayers, in unguarded moments, are likely to become a rarity. Nonetheless, Hamlet decides against killing the king in the hope that the chance for a much greater revenge will present itself. In the play, up to the culmination of this segment, the decision is one of selecting from equally plausible alternatives. To the audience either of the alternatives would make sense. Neither could be predicted on the basis of what we know about Hamlet and both are rational.

Hamlet's downfall may have been inevitable given the premises of the play. (If Hamlet did not kill the

king he would be guilt-ridden; given the ghostly presence of his father this would certainly be a "downfall"--Hamlet would have failed in his duty and be, as one says, "psychologically crippled." If Hamlet did kill the king he would be a regicide--an "outlaw.") But the way he fell is not "deducible" from the premises of the play.

In the play events "hang together" but the principle of their coherence is not the principle of cause-and-effect. The agents make decisions and suffer the consequences of these--there is this much of "cause-and-effect" about it; but the decisions are often, to the audience, reasonable decisions rather than inevitable ones. We see how the decisions fit the characters but we do not see that only these decisions could fit.

What Shakespeare does as a dramatist Strout thinks the historian must do as a matter of methodology. For Strout, one of the myths about the American Civil War is that it had to happen. But, he would argue, the American Civil War is not something over and above the men who engaged in it; it is the history of their engagement. They were not all mere puppets--they made decisions, some good and some bad, which gave shape to the emergence of war and

the nature of its culmination.

The historian should count himself fortunate if he can bring all this to life and give some coherence to what is called the American Civil War. The historian who, as a matter of principle, goes from "plausibility" to "inevitability" removes the dramatic quality from his history. Mechanically applying the dubious axiom, "Whatever happened had to happen," he rules out the contingencies in human events, the ironies, the absurdities, and the "unanticipatable" which Strout believes make history of such great interest to all of us.

The historian who rules out these things reads the course of human events as if that course were a planetary orbit. Just as Benson's historical perspective fits in with the covering law model of historical explanation so, it can be seen, Strout's dramaturgical perspective fits in with a model of rational explanation. For when, in a particular case, we do not believe that laws have unambiguous application we look for reasons, motives, and all the other things which are the stuff of drama. Of course, a convincing drama always presupposes laws. Only in the worst melodramas of the "old west" does the hero keep on

firing away with his six-shooter, without reloading, for more than six shots. Only in admitted fantasy, like Miracle in Milan, can the dispossessed of this earth escape persecution by flying off into the heavens.

Aside from mechanical limitations and physical impossibility there are laws more central to the human condition, as such, made use of in the drama. The playwright does no harm to the thesis of Strout when he introduces a situation in which the protagonist is depressed because he has been rejected by the people he admires. But the playwright is likely to do more than produce a study in depression or exhibit the causal connection between the hero's personality and his rejection by society. If he does no more than that he will be charged--as Tennessee Williams sometimes is--with writing his plays out of psychological textbooks. The stuff of drama does not reduce, without remainder, to principles drawn from psychological or sociological case studies.

The perspectives of Benson and Strout are attitudes toward history; these are not mere reports about how history is written; they are guidelines of a general description of how history ought to be written. These

perspectives, it seems to me, influence the analyst in his choice of what counts as an historical explanation and what is to be described as a "gap" in an historical explanation. For example, the "causal" theorist will not be satisfied with an explanation of Lincoln's behavior which terminates in a "reconstruction of Lincoln's thought." He will wonder why that thought took the shape that it did; he may look to psycho-analysis, or psycho-dynamics, for the causal principles in accord with which that thought can be seen to be explicable.

He will quite possibly think of any explanation of Lincoln's decision to provision Fort Sumter which does not at least suggest such a step as essentially "incomplete" or "not really an explanation." If he has no faith in psycho-analysis or psycho-dynamics he will look elsewhere for his covering laws but, however unsuccessful his quest, he will not stop looking.

On the other hand, if the "dramaturgical" theorist comes to the conclusion that Lincoln ordered the provisioning of Fort Sumter to force the South either to recognize federal authority or go to war he may be quite satisfied with that explanation as it stands. He may

believe it to be pointless to go beyond the decision. For one thing, the decision makes sense. It was one way--although probably not the only useful way--of bringing matters to a head in the dispute with the South. For another, the decision is creative. Not just any politician would have hit on just that kind of decision. Lincoln, after all, guaranteed that the North would not, in the North, be viewed as the aggressor. Had Lincoln ordered an invasion of the South, the tepid and the indifferent might have come to sympathize with the South. On the other hand, few northerners would have been inclined to question Lincoln's right to order provisions for a federal garrison. The South was put in the position of having to capitulate--or allow its independence to be "worn away"--or accept a situation in which southerners fired the first shot and thereby lost the moral advantage to Lincoln.

How is the historian to show that Lincoln's creativity in matters of politics or language is explicable in terms of covering laws? He can say there must be covering laws or believe in the prospects of learning theory or cybernetics. He can insist that a complete explanation--which he is in no position to provide--would constitute

the only ultimately acceptable explanation. The "dramaturgical" theorist will treat such insistence as evidence of a picture of human life (a metaphysics of history) in which human life is stripped of its dramatic quality; a picture which denies that men can do the unpredictable thing.

These attitudes toward history--"causal" or "dramaturgical," "positivist" or "idealist"--are capable of sustaining the shock of a particular piece of analysis which seems incompatible with the attitude; i.e., although Hempel may be able to show that a given explanation in history is really in the form of an explanation sketch, the "dramaturgical" theorist can always argue that such explanations amount to nothing more than a few details on the canvas of history or are even to be considered as no more than a backdrop to history.

Similarly, the fact that some explanations seem to be of the "rational explanation" form does not preclude the supporter of the "causal" perspective from saying that this kind of explanation is somehow inferior to the type of explanation found in the "hard" sciences.

These attitudes are not directly testable. They

sustain those who develop models of historical explanation but they are not refuted when specific models of historical explanation break down or are shown to have a more limited application to historical narrative than was claimed for them. The attitudes are "broader" than particular models and their defects. The continual revision of these models bears witness to the strength of the underlying attitudes. These attitudes, while not directly testable, might be measured in terms of their fertility; the insights into history which they permit, the understanding of history which they foster.⁶ It is true that these attitudes "stipulate what is to count as understanding"⁷ in history just as they represent a view of how history ought to be written but they are not meant to limit the scope of historical research (or, at any rate, narrow

⁶It will be seen that I treat "perspectives" as Karl Popper treats "interpretations"; I owe the point about fertility to his The Poverty of Historicism (Routledge and Kegan Paul, London, 2nd ed., 1960), p. 151.

⁷The formulation in quotation marks is taken over from Professor William Dray's criticism of the original version of this chapter--a criticism in which he pointed out that I had produced a circular formulation of the "fertility" notion.

historical research to any great extent).

If either of these "attitudes" or "perspectives" limit the scope of historical research, we cannot really say that they provide a satisfactory insight into history or foster an understanding of history; if the notions of what is to count as historical understanding incorporated in these perspectives force us to truncate history, we may well judge that the perspectives are not fertile enough. G. J. Warnock, in one of those biting passages which characterize his book on English philosophy, comments:

But metaphysical systems do not yield, as a rule, to frontal attack. Their odd property of being demonstrable only, so to speak, from within confers on them also a high resistance to attack from outside. The onslaughts of critics to whom, as likely as not, their strange tenets are very nearly unintelligible are apt to seem, to those entrenched inside, mis-directed or irrelevant. Such systems are more vulnerable to ennui than to disproof. They are citadels, much shot at perhaps but never taken by storm, which are quietly discovered one day to be no longer inhabited.⁸

A fine passage, in my opinion, but is it true? Did

⁸G. J. Warnock, English Philosophy Since 1900 (Oxford University Press, London, 1958), p. 11.

we tire of Cartesian dualism? Do we reject Hegel's philosophy because it would be too tedious to think about it? I should like to think that something other than boredom accounts for our rejection of a metaphysical system and something more than novelty accounts for our embracing one.

Surely metaphysical systems are at least sometimes rejected because they are thought to be wrong. I would think that hardly anyone would reject Spinoza's system because he found himself overcome with "ennui" when he encountered it. Surely it is possible that a man rejects Spinoza's system because he thinks that there is more than one infinite substance or because he thinks that the mind is more than an idea of the body or for any number of other reasons.

It seems to me that the same thing may be said about the "attitudes" I have been discussing. In fact, as I view them, they are "metaphysical perspectives" and if Warnock is right about metaphysics in general, I see no reason why he would be wrong about metaphysical perspectives in particular. But if we take up an attitude toward history--"causal" or "dramaturgical"--is it not possible

that we will abandon that attitude if we discover that it truncates history? While the attitude dictates what is to count as understanding in history, it cannot very well survive a demonstration that this dictation would restrict the scope of history. The metaphysics we accept, if we accept any (and I am, perhaps foolishly, one of those who thinks that every philosopher has one--even Warnock), is the metaphysics which illuminates our experience without denying any of it, or at least seems to accomplish this.

Similarly, the historical attitude we make our own will be the one that we think preserves the essence of history while making it clearer than any other perspective just what that essence is. To defend the general notion which I have been sketching out--that metaphysical systems are actually subject to the criteria of verifiability and falsifiability that we apply elsewhere--would take a book and I can hardly expect the reader to sympathize with a controversial view with such broad implications. But the reader may be prepared to grant that a historical perspective which limited history would be defective--somehow inadequate or even impotent. It is in this respect--in terms of fertility or adequacy or potency--that I would

say that historical perspectives can be, at least in theory, subjected to tests.

It is not so easy--if it is possible--to conduct a test in practice. To begin with, as White pointed out, "history" covers a wide range of activities. The concept "history" is not itself a technical concept. Everything from biography to the history of the world, from military history to the story of the development of culture, comes under the concept of "history" unless we insist--and why on earth should we?--that only what is taught in a history department is the genuine article.

It is, however, always possible--if never profitable--to insist that some area of what most people would call "history" is not really history. The "looseness" of the concept permits such denials and, no doubt, can confer on an historical attitude--whose plausibility may rest on excluding certain researches from the domain of genuine history--that "high resistance to attack from outside" to which Warnock refers.

There is also the further difficulty to which Dray, implicitly, called attention in his criticism of my conception of the notion of "fertility" as it might be

applied to historical perspectives. History books might be read in such a way as to support one or the other of the perspectives I have discussed.

But I do not think that these difficulties actually make historical perspectives immune to criticism. There is a point at which we decide the metaphysician is "saving appearances" at too high a cost. We cannot state at what precise point he goes too far in meeting a criticism or claiming that a criticism misses the mark. But somewhere along the line the game is up. Few metaphysicians are like the woman William James discusses in one of his writings--a woman who decided that the two essential ontological categories were those of the "thick" and the "thin." But the principle is the same. We are not bored by her "discovery." We do not enter the castle of her system and find it uninhabited. We simply laugh or shrug our shoulders and wonder if she founded a religion in Los Angeles.

Why do we do this? Surely it is because we find her system poverty-stricken. We might as well divide the world into the "tall" and the "short," or the "dainty" and the "dumpy." I think the same thing applies to histor-

ical perspectives. But it does not apply in any obvious way. There are the difficulties I have already mentioned. There is, moreover, the difficulty of thinking out a history which would be faithful to one, and only one, of the perspectives I have discussed. One must perform a thought-experiment. What kind of history would we have if we had, say, a purely Hempelian history? Hempel thinks we already have this kind of history, but if the reader agrees with what I have said so far he will certainly take Hempel's view to be, to say the least, "overly optomistic." What kind of history would we have if we had a purely Dravian history? Dray, unlike Hempel, is prepared to admit that actual histories have a law-like as well as a rational dimension,, although I think it is fair to say that he thinks the rational dimension is the more important of the two. Still, recognizing that Dray is one of those who would not accept either historical perspective, without qualification, as his own, we can ask what such a history would be like (that is, one in which only Dravian explanations were employed).

It seems to me that when we answer these questions we find ourselves excluding either the rational or the

non-rational dimension of human behavior. The Hempelian history would be one which had no room for those individual responses which would separate out the reactions of a human being from those of a physico-chemical mechanism with a "coded" chromosomatic blueprint and a range of behavior dictated by that blueprint plus the environment. There would be no room for "appraisals" except in the sense that the historian, as a physico-chemical mechanism, might have to make "appraisals" because of his make-up. If an individual act escaped the mesh of laws, it would be only because we did not know enough about the applicable laws or because--here, I take it, "probabilistic" explanations would come in--the individual had a "random element."

Now I know full well that Hempel does not talk of "coded" genetic structures and random elements, but if he were to consider the kind of history, and its implications, that he has in mind, I think he would be led to talk of these things. Surely if all human actions come under laws there is no scope for what we, in our daily life, think of as men. To be sure, Hempel talks of rational actions, but these turn out to be psychological dispositions. Their character as rational acts is sieved away in their sub-

sumption under covering laws. A man's rational acts are no less a result of his psychological make-up than his irrational acts. We maintain the distinction but no longer with the implication that rational actions, unlike non-rational, or irrational, ones are the result of choice.

Perhaps even that would not matter if it were not for the peculiarity that, if we follow Hempel, we must always suppose that behind every rational act there is a law. Why may not rational acts, whether or not the result of "free will," ever be terminal? Surely there is an unproven metaphysics behind Hempel's conception of history. It is, in my opinion, the metaphysics of man as a machine. This is hardly the place to refute the metaphysics--assuming it can be refuted--but it is worth mentioning when we try to consider the future of a Hempelian history. Such a history would be a history of drives, stimulus-response mechanisms, conditioning theory, and things of that sort. A history of this type would be, I think, possible, but it would force us, as Berlin pointed out, to revise or abandon a great many of our concepts or--if we preserved them--to preserve them with a guilty conscience. Moreover, this theory of history carries the unwarranted implication

that scientific explanation is somehow complete, whereas rational explanation is somehow incomplete. If we do eliminate rational explanations from history--for the sake of a scientific history--we have no reason for doing so on the grounds that they are defective. We would legitimately eliminate them only on the grounds that we decided it was best to view man as bound by laws and were not concerned with the kind of information rational explanations furnish.

If we imagine a purely Dravian or Collingwoodian kind of history, we again find ourselves eliminating an aspect of man's existence we have come to know well--the aspect of the irrational and the non-rational. A history of the persecution of the Jews in Germany which concentrated only on the goals of Hitler and the Nazis would seem to me to be a defective history. It is not that Hitler did not have reasons for selecting a scape-goat. It is not that the average German was in much of a position to evaluate anti-Semitic propaganda. Certainly one can find reasons for the hatred of the Jews. But this, as with persecutions in general, is only part of the story. Some people, it would seem, hate on command. Their hatred may very well have historical consequences. A history which

left out habits and hatreds, the unreasonable and the insane, the effect on a populace of brainwashing and conditioning in general, would seem to me as much a distortion of the historical dimension of life as Hempel's notion that laws somehow--knowable or unknowable, vague or less than vague--stand behind every human action. Indeed such a history might do more harm by causing us to overlook the sometimes non-rational, or irrational, springs of our thoughts and actions. Hegel is much mocked for his statement that "The real is the rational and the rational is the real," but we should come very close to his position if we made our history into an affair of purely rational explanations.

When we ask ourselves what kind of world is implied by a purely Hempelian or a purely Dravian history, I think we can see that the implied world is not really the world of our experience. Perhaps we misinterpret the world and what we mean by our "experience" is too theory-laden a notion. But we can work only with what we have. If one or the other of the perspectives I have considered (I do not wish to say that these are the only possible perspectives but they are the ones relevant to the general discussion

in this monograph) goes against our notions of what the world is like we seem forced to reject the choice: one or the other, but not both.

In conclusion, I might say that I realize that I do not prove that what I call "historical perspectives" have actually guided debates in the philosophy of history. About that, I have two things to say. First, my hypothesis does make sense of the fact that the debate has persisted for a long time. It also makes sense of the charges one often hears that one side or the other misreads history. Furthermore, it makes sense of the fact that even when a model is made to appear very implausible, it has descendents (for example, weakened forms of the covering law model). Other hypotheses may be much more satisfactory than mine--may both counter and demolish mine--but I think that the topic of this chapter is worth discussion and I should like to see further investigations.

Second, in the next chapter I hope to provide, on semantic grounds, a defense of the position that the kinds of explanation discussed in this monograph are equally acceptable in history. I, personally, think that the metaphysical and the semantic considerations quite

properly go together, but I recognize the fact that some readers will find semantic considerations more philosophically "discussible" than metaphysical considerations.

CONCLUSION

CHAPTER IX

THE METHODOLOGY OF HISTORY

History, as it is and has been practiced, does not have one methodological principle of explanation. Some human acts are treated as subsumable under laws (whether or not the laws are known) and other human acts are treated as exemplifying a principle of rationality. Just how much of human behavior is to be classified as non-rational and how much as rational is a matter which calls for a decision on the part of the individual historian. There is a good deal of behavior we would all classify as irrational (whether the behavior is considered to be historically significant or not) and many of us are coming to accept the view that much of human behavior, although not necessarily irrational, is non-rational. But the classification is not a clear-cut one. In this respect, again, history differs from the sciences, pure or applied, with the exception of

the social sciences. An act which one historian may find to be non-rational, the reflection of habit or conditioning, another historian may find to be quite rational and another may find to be non-rational in that subclassification of the non-rational we refer to as the "irrational." Some historians would classify Machiavelli's forgery of the signature of Caesar Borgia as the rational act of a man who, seeking to amuse himself, wished to see if he could make a good copy of the signature. Others would classify this forgery as an irrational act on the grounds that Machiavelli must have succumbed to a power phantasy without realizing what he was doing.¹ The evidence presently available lends itself to both interpretations and which interpretation an historian accepts seems to reflect the degree to which he agrees with certain theories of psycho-analysis.

However, despite the differences we may expect to find in individual historians concerning the boundaries

¹See Renzo Sereno, "A Falsification by Machiavelli," Renaissance News, Vol. 12, No. 3, 1959. Sereno takes a psycho-analytic approach but discusses, briefly, other interpretations of the forgery.

of the rational and the non-rational (including the irrational), it is generally true to say that each historian sees his explanatory patterns as being grounded in evidence and subject to refinement or abandonment; i.e., further developments in economic science or the discovery of a hitherto unknown diary may cause the revision--or rejection--of these explanatory patterns. There is an evidential check just as there is in the exact sciences. Attacks within the historical profession upon a given work of history usually rest upon considerations of new evidence, allegations that the historian has overlooked "old" evidence, or claims that other interpretations of the data are possible. No historian is likely to be attacked by another simply because he has given a "rational" explanation or taken for granted the existence of a "covering law" explanation.

The reason such attacks are unlikely is brought out very well, in the course of a discussion of Freud's theory of the unconscious, by A. C. MacIntyre:

Freud's theories fall half-way between two brilliant but wrongheaded monolithic attempts to account for human behaviour, which have shared between them the task of

correcting the Cartesian theory of mind. On the one hand there is the view that Descartes was right in seeing the physical world as a world of mechanical causation, wrong in seeing the mental world as exempt from its impact except through the slender medium of the pineal gland. What has to be done is to assimilate the explanation of mental states and occurrences. The eighteenth-century exponents of this view such as Diderot and de Lamettrie find their contemporary heirs in the American "behaviour theorists" such as Tolman and Hull whose various theories of learning seek to exhibit behaviour as consisting in a set of responses to external stimuli, the nature and quality of the response being determined by predisposing causal factors. These theories have so far only found a whole precise exemplification in the behaviour of rats, but their application to human beings has generally been considered by their exponents as only a matter of time, effort and experiment. At the other extreme, there are those who admire the Cartesian autonomy of mind so much that they regret any suggestion of dependence upon or interrelation with the physical at all. Such are the French existentialists of the present day, Sartre and his disciples. For Sartre all important human behaviour is the fruit of human decision. You are what you are because of what you have decided. This is asserted not just of actions but also of attitudes and emotions. Your sadness is the result of your choosing to be sad. There are no antecedent conditions which determine human behaviour. The difficulty with both of these positions is that each conflicts with some quite undeniable feature of human behaviour. For clearly on the one hand the work of appraisal and argument

that goes on in the mind affecting and altering all our conduct can never be adequately described in terms of stimulus and response if only because--A Kantian insight--the validity of reasons as reasons cannot be dealt with in causal terms. On the other hand a great deal of the life of the mind clearly takes the shape that it does because of formative environmental and biological conditions. But equally clearly each of these positions states a legitimate starting-point for approaching human behaviour. One may ask "Why?" and expect an answer in terms of reasons, intentions, purposes and the like; or one may ask "Why?" and expect an answer in terms of physiological or psychological determining antecedent conditions. This dichotomy remains untouched when the misleading character of other dichotomies such as that between the mental and the physical, or the inner and the outer aspects of human behaviour, has been noted.²

The practicing historian is prepared for answers to a "Why?" in terms of--to enlarge upon MacIntyre's remarks--psychological, sociological, or economic determining conditions, but he is also prepared for answers to a "Why?" which are put into the terminology of "reasons, intentions, purposes and the like." "Monolithic attempts to account for human behaviour" do not hold him captive.

³A. C. MacIntyre, The Unconscious (Routledge and Kegan Paul, London, 1958), pp. 50-2.

This is to say that within the historical profession, "explanation" (in the sense of explaining why) has at least two equally acceptable uses.

The analysts of historical explanation have really shown that the use of "explanation" varies from one context to another; they have not shown that there is a "hard-core" invariant use. They have been unable to do this because "explanation" is not a syntactic or a semantic term defined, or given the rules for its use, in some artificial language system. "Explanation" is a pragmatic term, like "testable" or "confirmable," having reference to a speaker of a language:

In so far as there are different respects in which one can be said to lack understanding of an act, a condition, a tendency, a law, etc., so far there are different ways in which it can be explained. For historical explanation this does not have the consequence that explanations are judged by some purely subjective standard of empathetic acceptability, since to say an historical phenomenon is understood is not to say someone (or everyone) thinks he understands it. There are objective tests for understanding just as for knowing or inferring. These happen not to be syntactical tests as are (supposedly) those for deducing; but

then explanation is not a syntactical but a pragmatic notion.³

Rules for the use of "explanation" must be rules which a speaker of the language will accept if he is confronted with them. If an "explication" of a term is such that no user of the term would accept the "explication," the alleged "explication" is really a stipulation which people are free to accept or to reject.

Broadly speaking, we may say that analysts have articulated two sets of "rules" for the use of the term "explanation"; since the rules constitute distinct sets, it will be convenient to assign subscripts to the term "explanation" and speak of "explanation₁" and "explanation₂." The rules for the use of "explanation₁" are those of covering law theory. The rules for the use of "explanation₂" are those involved in producing a successful rational explanation.

"Explanation₁" and "explanation₂" are both exemplified in historical practice. ("Explanation₁" is

³Michael Scriven, "Truisms as the Grounds for Historical Explanations," in Patrick Gardiner, ed., Theories of History (The Free Press, Glencoe, Ill., 1959), p. 452.

exemplified--if my general argument is sound--primarily, if not exclusively, in the weakened form of Hempel's model I have previously developed.) Historians recognize both as equally legitimate modes of connection in historical narratives.

However, a number of philosophers of history have maintained that either "explanation₁" or "explanation₂" is the explanatory pattern employed by practicing historians. The exclusive sense of "or" is puzzling. It seems to rest on a belief that the concept "explanation" either has an essential character (Hempel certainly has this belief) or, when it is a matter of explicating the behavior of human beings, has only one proper use. (The "idealists" do not deny that there is such a thing as scientific explanation. They grant that there is more than one kind of explanation, whereas Hempel does not. But some of them, such as Collingwood and Donagan, restrict the use of "explanation" in history to a non-scientific form.)

To clarify this point we may distinguish between "external" and "internal" questions.⁴ An "external"

⁴This terminology is taken from Rudolf Carnap's

question is a question about the legitimacy of a given linguistic usage. The question, "Are classes real?" is an external question about the legitimacy of a system which employs class notation. The questioner may well know that there are such systems but may be wondering whether or not there ought to be. He may think that if "classes are not real"--whatever "real" may be taken to mean--any language which employs a class notation is misleading unless the conventions governing the language make it possible to replace class-terms by individual-terms through some process or another.

An "internal" question is one about the nature of a given language. "Is Goodman's logic 'nominalistic'?" is an internal question because the questioner just wishes to know something about the structural features of Goodman's system. He is not, in any way, calling the system itself into question.

It is apparent that if the question, "Do historians

"Empiricism, Semantics and Ontology," reprinted with modifications in Meaning and Necessity (Phoenix Books, University of Chicago Press, Chicago, 2d impression, 1958), pp. 205-221.

use instances of explanation₁ or explanation₂ but not both?" is taken to be an internal question, its answer is in the negative. The theorist can only attempt to limit the historian to one and only one explanation pattern on the basis of an answer to an external question. For example, the question, "What would be the most suitable use of 'explanation' for the historian?" is an external question. The questioner has asked himself what use of the term "explanation" would be best for history. And what counts as "best" is likely to be governed by a concern for the future of history--for the kind of history the questioner would like to see, or hopes to see, come into being.

An answer to the question will be not only a recommendation about the future use of a word but also a recommendation about what historians should aim at. There seems little question that a concern with the future of history has played a role in the debate between the "idealists" and the "positivists." This concern has been expressed by William Dray:

My chief complaint against acceptance of the covering law doctrine in history is not the difficulty of operating it, in either deductive or mutilated form. It

is rather that it sets up a kind of conceptual barrier to a humanistically oriented historiography.⁵

Dray is willing to allow that history is a mixture of art and science; he does not insist that covering laws can have no place in history, but he does not wish to see the day when history might develop into a science. For him, as for Berlin, too much would be lost. History would lose, I think Dray would say, those qualities I have referred to in discussing the "dramaturgical" perspective of history.

The chief objection against the "idealistic" as position (as represented by Collingwood and Donagan in a form that recognizes no covering law explanations as having a place in history) is that it sets up at least a partial conceptual barrier between history and the sciences; and if we agree with the "idealists" we discover that the "methodological unity of science" does not encompass history. This objection--implied by Hempel in his "The

⁵William Dray, "The Historical Explanation of Actions Reconsidered," in Sidney Hook, ed. Philosophy and History (New York University Press, New York, 1963), p. 133.



Function of General Laws in History"--is tied to his answer to the external question of what ought to count as an "explanation."

But why may we not simply accept the idea that "explanation" has two uses in history? We can accept this idea without begging the question of which use will prove--or does prove--most important for the historian attempting to explain the past.

It may be said that strictly "positivistic" or strictly "idealistic" explications of the nature of historical explanation have a recommendatory character--the "positivist" and the "idealist" actually are suggesting that one use of the term "explanation" (whether or not--as with Hempel--it is claimed that there really is only one legitimate use of the term) will prove best for the purposes of history. It is not enough, it may be argued, to show that historians may accept both "explanation₁" and "explanation₂" in order to deal with the "positivists" and the "idealists." One must consider the value of the recommendations the "positivists" and the "idealists" put forward.

I think this line of reasoning is correct. It

is not good enough to show that historians accept two or more uses of the term "explanation" in order to show that the strict "positivist" or the strict "idealist" conception of history distorts history. Some recommendations do, after all, turn out to be good ones. We must ask: what benefit would accrue to the historian if he accepted the recommendation of Collingwood or that of Hempel?

I think the answer is that no benefit at all would accrue to the historian. Whether or not I am right about this depends upon the question of whether or not I am right in thinking that both the "causal" and the "dramaturgical" perspectives of history capture something of the subject matter of history without either, exclusively, capturing the essence of history.

If I am right about this--if it is true that history, in reflecting more of the "concrete" aspects of human existence, has both a rational and a non-rational dimension with which to deal--the historian, who wishes to preserve the scope of his discipline (and why should he not?), has nothing to gain in consciously seeking to be a Collingwoodian or a Hempelian. The external question of the proper use of "explanation" in history loses any hold

on the historian when he realizes that any answer to this question will limit the scope of his practice if he embraces that answer.

As for the internal question, I have already said that historians, like other people, recognize more than one kind of explanation or more than one use for the term "explanation." They need hardly feel that there is anything superficial about this. In science, we explain things--wherever possible--by means of laws. In daily life we explain some things in accord with laws (for example, personality deteriorations, various passions, and notions which seem to be the result only of conditioning) and other things in accord with reasons or intentions and purposes. Why should not the historian--who takes the human world for his province--do what we all do?

History takes as its province the study of human actions before these actions have been subjected to a treatment of abstraction for the purposes of the various behavioral sciences. The historical man is simply a man whose actions are of historical significance. He is not the "economic" man or the "member of the peer group" or a mere instance of any one of a number of classifications.

This hardly implies that the behavioral sciences can throw no light on human actions which are of interest to the historian. But history is not like an applied science because it takes man (or, more accurately, the man of historical significance), as man, for its province.

We may produce a scientific history--at least we may suppose that this is possible--but the production of such a history would entail a limitation of the subject matter just as the production of a purely Collingwoodian history would also entail a limitation of the subject matter (or, if the reader prefers, a redefinition of "history").

If we use the term "history" to refer to all those investigations which an historian would call history, we are forced to conclude that history is a mixture of scientific and rational modes of explanation. No doubt there is much more to the methodology of history than that but there is at least that.

If one may hazard a guess about the future of history, as likely a guess as any about that future is the assumption that history will become progressively more like the sciences in its employment of generalizations--if

only because it is more likely to borrow generalizations from the sciences as these, in their turn, develop more well-attested "covering laws"--but will hardly lose that character which has made so many thinkers associate it--in its "concreteness"--with the literary arts.

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